Nova School of Business and Economics

Universidade Nova de Lisboa

Dissertation, presented as part of the requirements for the Degree of Doctor of Philosophy in Economics | Finance

Essays on Corporate Finance and Political Economy

Diogo Gonçalo De Matos Mendes, Nr. 5

A Dissertation carried out on the PhD in Economics | Finance, under the supervision of Professor Miguel A. Ferreira

September 2, 2019

Abstract

The first chapter of this dissertation investigates how financial frictions affect companies' product market decisions. As different products have different production cycles, I find that companies focus on products entailing short cash flow maturity as a way of alleviating financial constraints. The second chapter focuses on a randomized controlled trial where a financial education course was offered to managers of medium and large companies in Mozambique. The results suggest that financial education improves corporate practices and performance. The final chapter analyses the impact of information on electoral behaviour. We expose 1800 participants to information about central government performance and evaluate its impact on voting in local elections.

Keywords: financial constraints, product market decisions, financial education, voting behavior

Acknowledgements

I thank all the support and friendship of my advisors Cláudia Custódio and Miguel Ferreira. They showed me research in finance is far more than studying random walks in financial markets and provided me unconditional guidance when my walk throughout this journey was following random paths.

I thank my host university, Nova School of Business and Economics, for giving me all the opportunities to pursue my academic goals. I also acknowledge other schools I visited, namely London School of Economics and Imperial College Business School, where I met people who contributed a lot to this work.

To the extraordinary companionship of my friends who, in their own way, made this a very pleasant adventure. My gratitude goes to those who shared with me the pearls of being a Ph.D. student, namely João, Marta, Matilde, Mattia and Pedro. It also goes to others that know very little about this thesis but showed me how to enjoy life outside the office. Thank you, Daniel, José, Mariana, Marta and Miguel.

To my parents, grandparents and brother. Words cannot describe the importance you had in the last half a decade.

Lastly, but foremost, to my girlfriend who has always been by my side in the bad and goods moments of this long journey.

To all, *Muito Obrigado*.

This work received the support of Fundação para a Ciência e Tecnologia (FCT) under the grants PD/BD/105722/2014 and PTDC/IIM-FIN/4177/2014.

Contents

Introduction

The first chapter of this dissertation studies how financial frictions affect product market decisions of companies. Different products have different production cycles and generate cash flow at different maturities. Therefore, companies may adjust their production cycles to alleviate financial constraints. I use the wine sector in Portugal as a laboratory because product mix decisions can be easily identified and linked to cash flow maturity. I exploit a banking regulatory shock which impacted negatively on credit availability, and I find that credit constrained firms change their product mix in response to the shock. Firms shift from long cash flow maturity products to shorter ones. My results suggest that the adverse impact of financial constraints on product markets may be exacerbated with longer, less-flexible, production cycles.

The second chapter studies the impact of a financial education program for top managers of medium and large enterprises in Mozambique through a randomized controlled trial (RCT). Using survey and financial reporting data, we find consistent evidence that managers adjust financial policies in response to the education program. The largest treatment effects are on short-term financial policies related to working capital, which generate a positive impact on cash flows due to reduction in accounts receivables and inventories. There is also a smaller but significant positive impact on long-term investment. These firm policy changes improved firm performance of treated firms. Overall, the results suggest that relatively small and low-cost interventions such as a short exec-

utive education program in finance improves financial practices and can affect economic development.

The third chapter analyzes the role of information on electoral outcomes. A substantial part of the information spread out around elections relates to central government performance, even when it is not directly related to the election taking place. We investigate whether the perception about central government performance drives voting in local elections. We conducted a randomized controlled trial around the Portuguese 2017 local elections: 1800 participants were exposed to information about central government action. We find that negative information has a strong impact on performance perception (negativity bias), and that this impact depends on the *ex-ante* level of awareness. Using the exogenously-induced change in perception, we investigate voting outcomes at local level. We find no treatment effect on turnout or probability of voting for the government incumbent party in full sample. Nevertheless, our evidence suggests that central government performance perception plays a role among individuals with less rigid voting preferences.

Chapter 1

Financial Constraints and Product Market Decisions: the Role of Production Cycles

I thank Cláudia Custódio, Miguel Ferreira, Raj Iyer, Marcin Kacperczyk, Daniel Ferreira, Fernando Anjos, Irem Demirci, Diana Bonfim, Franklin Allen, Christopher Hansman, Daniel Carvalho, João Granja, Ernst-Ludwig von Thaden, Nadim Habib as well as conference and seminar participants at Nova SBE Lisbon, Bank of Portugal, and QED Doctoral Workshop for very helpful comments.

1.1 Introduction

Financial frictions may impact project maturity decisions of companies. Those decisions may span several dimensions such as investment in fixed capital (Campello et al., 2010; Cingano et al., 2016; Bucă and Vermeulen, 2017; Amiti and Weinstein, 2018), innovation (Howell, 2017; Krieger et al., 2018) or product market outcomes (Matsa, 2011; Phillips and Sertsios, 2013). In all these dimensions, inefficiencies may arise as companies favor short-term projects which provide short-term cash-flow at the expense of the long run. These inefficiencies are likely to have real implications for the economy.

Absent financial frictions, financing decisions should not affect investment decisions. However, as different investments may impact on cash conversion cycle, the need for internal funds might push for different - eventually sub-optimal - decisions (Maksimovic and Titman, 1991; Chevalier and Scharfstein, 1996). In this paper, I study whether financial constraints prevent companies from taking long investments.

Despite the importance of this question, the empirical evidence is limited. First, it is difficult to observe project maturity. Second, investment decisions are endogenous with respect to financial conditions as financial and investment decisions are jointly simultaneously taken (Giroud et al., 2011). Last, the relation between the two might exist because they co-move with another factor (e.g. market competition). These challenges, together with limited data availability, undermine identification.

In this paper, I overcome these challenges using a unique setting. I focus on product mix decisions in the wine industry. The Portuguese wine sector is a suitable laboratory to test my research question. Granular data on product market outcomes, such as product mix and product quality, is usually unavailable.[1] In this setting, I can accurately

[1]To circumvent these obstacles, existing empirical work has focused on specific industries for which granular data is available (Argente et al., 2019).

observe product mix decisions. Second, there is a direct link between product mix and companies' project maturity. While the production process of different wines is similar regarding inputs and technology, different products involve production cycles of different lengths due to the wine ageing process. In this framework, I can identify how product mix decisions translate into companies' project maturity. Specifically, I exploit industry regional regulations imposing minimum ageing periods on the production of certain types of wine. The combination of these characteristics allows to isolate and test my research question.

The focus on this sector overcomes a few other limitations. First, harvest and production occur only once a year. Thus, it allows to identify the beginning of each production cycle and match it with the frequency of financial reporting (Lovell, 1961). Moreover, this is a market composed by non-listed, private, small and bank-dependent companies. Existing studies tend to look at large or publicly-traded corporations.[2] I depart from this approach by focusing mostly on small and medium enterprises. According to Eurostat, around 99,8% of non-financial active enterprises within EU-27 were SMEs in 2008, that jointly accounted for 59% of value added.[3].

In order to observe companies' product mix decisions, I make use of the standardized classification scheme for agricultural products and foodstuffs in place in the European Union. The Protected Designation of Origin (PDO) and Protected Geographical Indication (PGI) certifications are attributed to several agricultural products such as wine, cheese and fruit, with the goal of promoting and protecting the reputation of regional products. Regarding wine product, PDO and PGI certified products are associated with higher quality standards due to more rigorous production methods and certification processes. These two product categories contrast with a third-category -

[2]Kashyap et al. (1994) examine micro data on US listed firms' inventory behavior but note that, if available, they would prefer to look at non-traded firms as these are most dependent on bank financing.

[3]Pocketbooks (2011).

non-classified wine, which is not subject to specific regulation or rigorous certification processes. Based on these three product categories, consumers can form an expectation about products' characteristics. Average prices of each product category illustrates that the market perceives them as different products in several European countries. This approach departs from other papers on wine value which use subjective measures such as experts' ratings or auctions' realized values (Gibbs et al., 2009; Dimson et al., 2015). Besides the subjectivity, those metrics only allow to study wines of famous winemakers, which poorly reflects the entire industry.

Evidence from the 2008-financial crisis suggests a link between financial leverage and product mix decisions. Figure 1.1a shows a decline in the production of PDO wine when the severe international crisis hit the Portuguese economy (2010). Performing a separate analysis according to levels of leverage as of 2008, low- and high-leverage companies follow different trajectories (figure 1.1b). I observe that the former group reached the average pre-crisis benchmark two years later while a much persistent and lasting impact affected highly-levered companies.[4]

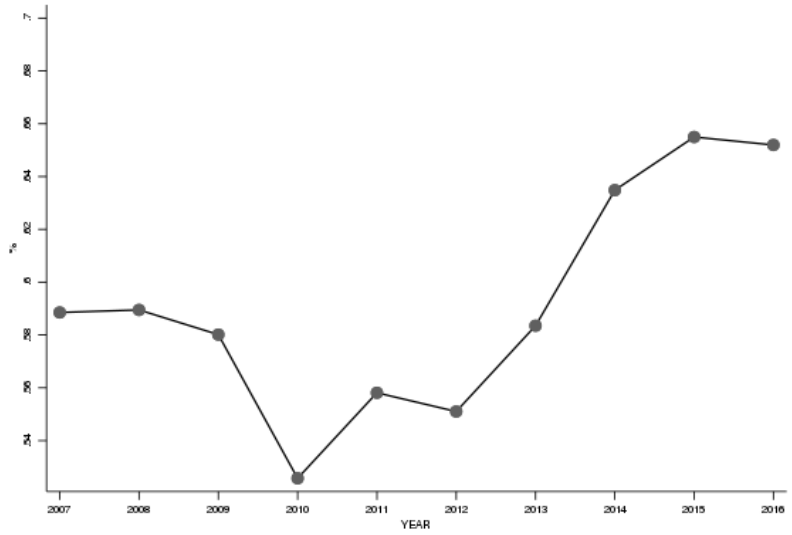
I exploit a source of exogenous variation in financial constraints to address the concern that financial constraints and product market decisions are simultaneously determined. In October 2011, the European Banking Authority (EBA) announced a Capital Exercise which required a subset of European banks to reach a 9% core tier 1 (CT1) capital ratio by June 2012.[5] This regulation in the banking system aimed at creating "an exceptional and temporary capital buffer to address current market concerns over sovereign risk" (EBA, 2011). This regulatory measure is arguably suitable as a quasi-

[4]I do not exploit the financial crisis as a quasi-natural experiment because all agents were likely affected and it poses challenges in identifying a suitable counterfactual. Moreover, the crisis might have created distortions in consumer preferences or market competition, which does not allow to clearly identify the impact of financial constraints.

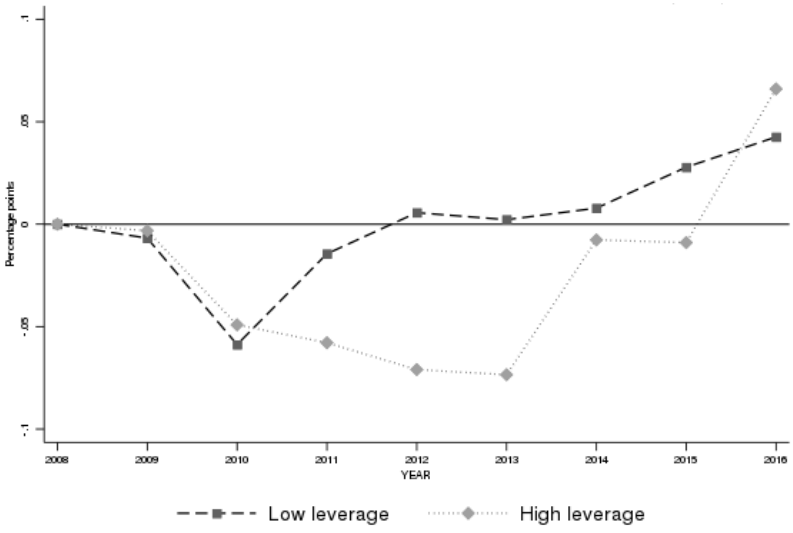
[5]This represented a sizable increase relative to the previous 5% requirement established under Basel III.

Figure 1.1: Evolution of PDO certified wine production

This figure exhibits the evolution of PDO certified wine production. Subfigure 1.1a depicts the evolution of PDO certified wine production in percentage of total production. Subfigure 1.1b shows the evolution relative to pre-crisis levels (2008) for high- and low-leverage companies. A company is defined as high (low) leverage if total assets over total liabilities are higher (lower) than the median.



(a) In percentage of total production



(b) Relative to pre-crisis levels (2008)

natural experiment for several reasons. First, it was largely unanticipated by economic agents (Gropp et al., 2018; Blattner et al., 2018). Second, affected banks responded

to higher capital requirements by cutting back on credit to the economy (Mésonnier and Monks, 2014). Finally, the exercise targeted banks in descending order of their market shares in each Member State such that it affected only the largest banks in each country. This generates a well-defined counterfactual.

I use the share of credit from affected banks as a measure of companies' exposure to the shock. In accordance with previous studies that have studied the impact of the EBA Capital Exercise (Mésonnier and Monks, 2014; Blattner et al., 2018), I find that companies in our sample with high exposure to affected banks faced a credit contraction following the shock. Unsurprisingly, I find that this impact is stronger for companies that rely more on bank loans as a source of financing. I use a triple-difference specification as the main empirical approach, where treatment is given by exposure to affected banks and a high pre-shock level of bank dependency (Amiti and Weinstein, 2018). This approach allows to compare bank dependent companies with high exposure to affected banks (*treatment group*) with companies that are not exposed to affected banks or not dependent on bank credit (*control group*).

I find that companies adjust product mix as a response to the credit contraction induced by the EBA Capital Exercise. Specifically, affected companies reduce the percentage of PDO wine (the highest certification category) on total company's production. The impact is economically and statistically significant. An increase in the share of credit from affected banks by one standard deviation decreases the percentage of PDO wine by 5 to 7 percentage points on average. I test whether companies adjust also in other margins as a response to the shock, namely total quantity produced and sale of grapes right after harvest. In the face of financial constraints, companies might adjust production levels as a way of cutting production costs, or generate immediate cash flow by selling grapes right after harvest to other producers. I do not find any statistically significant differences in those dimensions.

Next, I study the role of production cycle. In other words, I analyze whether the reduction in the production of PDO wine aims at shortening cash flow maturities. To do so, I split companies according to regions where there is a minimum ageing regulation on the production of PDO wine. These regulations establish a lower bound on the production cycle length. In regions where a regulation is in place, companies need to shift production of the constrained product to other products in order to produce faster. I show that the reduction in PDO wine production found in the full sample is entirely driven by companies operating in these regions. An increase in the share of credit from affected banks by one standard deviation is associated with a reduction of 10 percentage points in the production of PDO wine in these regions. The contrasting results among the two types of regions suggest that the ageing restriction plays a role and constrains companies in their product market decisions. Nevertheless, these findings rely on the assumption that these regions do not differ in other dimensions beyond the existence of an ageing regulation. To better understand whether the ageing restriction is indeed the driving factor, I exploit the fact that these regulations apply mainly to PDO red wines. By looking at within-company decisions of producing PDO red and white wine, I show that companies only adjust production of PDO red wine. This finding supports the role of production cycle mechanism.

A more direct way of alleviating financial constraints and generating cash-flow in the short-term could be through an increase in sales by selling current inventory (fire sales). Hence, I expect the adjustment in production to be stronger among companies with lower inventories. I find evidence that this is the case by splitting companies with high and low inventory levels. These results point towards a complementarity between production and levels of current stock. As managers see lower inventory levels, they seem to adjust production in order to produce faster and replace inventories at a faster rate.

Last, I discuss performance implications of such decisions. I analyze whether affected companies that have adjusted their product mix perform better than affected companies whose production remained unaltered. Although this step is not clearly identified because adjustment in production is an endogenous decision, it may give some indication about the direction of the effect. I find that companies that adjust the production of PDO wine downwards present higher performance indicators following the shock. Overall, this result seems to suggest that the adjustment in production was an optimal response to the shock.

I perform several robustness tests. Alternatively to the minimum ageing restrictions, I proxy the duration of the production cycle with a financial measure of inventory duration. Consistent with the need to shorten production cycles, I find the adjustment in product mix is stronger among companies with high days sales of inventory. I try to investigate whether the response can be due to certification costs. If certification costs are a driving mechanism of the effect, I expect companies to shift production from PDO to non-classified wine (the product category that does not undergo certification processes). I show that the reduction in PDO wine is accompanied by an increase in the production of PGI wine. My results are also robust to alternative definitions of treatment and different time periods.

The choice of focusing on a single industry comes at the expense of external validity, as results may not easily apply in other industries. However, the production cycle mechanism may be a key driver of product market decisions in several settings. Straightforward cases include industries where different products require long production periods. Some examples include forestry activities, livestock production and certain types of foodstuff, such as wine or cheese.[6] Nevertheless, this effect may extend beyond

[6]Regarding investment in woodland, in March 2017 Financial Times wrote '*"There is a frightful interval between the seed and the timber."* So said the 18th-century wit and writer Samuel Johnson, but wait patiently for your forest to flourish and you could see a significant return on your investment.'

inventory-dependent sectors. Activities in which production entails large upfront costs and long development periods such as innovative industries or the construction industry are suitable examples. Under financial constraints, companies may decline positive NPV projects when the estimated time to develop it is long. It may extend to the academic profession as well. Researchers may favor shorter research agendas, such as the ones that do not involve collecting (or waiting for) data over long periods, something that in many cases is out of their control. Finally, financial constraints may affect product market decisions through the production cycle mechanism in industries such as mining or oil extraction. Gilje et al. (2017) show that financial constrained companies anticipate completion of oil wells at the expense of higher long-run returns. This behavior, which aims at relaxing financial constraints, requires anticipating production and thus it also fits in the definition of production cycle mechanism.

This study contributes to several strands of the literature. First, this paper contributes to the literature on the link between financial constraints and corporate decisions. Based on the pecking-order theory of capital structure, Fazzari et al. (1988) find evidence that corporate investment is not independent of financing decisions. Since then, the literature has studied relations between financial constraints and specific investment dimensions. Bolton and Scharfstein (1990) show that high leverage constrains a firm and leads to suboptimal investment. Maksimovic and Titman (1991) argue that high levels of debt adversely affect investment in quality as managers favor short-term cash flow to avoid financial distress. Their intuition finds support in the debt overhang problem developed by Jensen and Meckling (1976) and Myers (1977), i.e. managers have incentives to compromise long-term returns as any future loss is shared with debtholders. A different approach is discussed in Chevalier and Scharfstein (1996). Using a model of countercyclical markups, they show companies may opt to generate earlier

https://www.ft.com/content/44153aae-0039-11e7-8d8e-a5e3738f9ae4

cash-flows because they are not able to invest. Second, I investigate whether the length of the production cycle impacts product market decisions. A glimpse of what may be explained in the light of this mechanism is briefly addressed in Matsa (2011). He finds that the adverse effect of leverage on product availability in supermarkets is higher for vertically integrated supermarkets, i.e. the ones that control a substantial part of the productive process. Although suggestive, one cannot claim this finding arises from the mechanism I propose though.[7] To my knowledge, this is the first study to investigate the role of this mechanism in product market decisions. Finally, I assess if in the context of industries with long inventories, production and sale decisions complement each other. To some extent, my results add to the literature on inventory dynamics that has studied relations between inventory behavior and several economic dimensions, such as industry competition (Rotemberg and Saloner, 1989), financial prospects (Carpenter et al., 1998), and reputation (Blazenko and Vandezande, 2003).

The remainder of the paper is organized as follows. Section 2 describes the setting. Section 3 describes the data, empirical methodology and presents descriptive statistics. Section 4 presents the results. In section 5, I show additional robustness tests. Section 6 concludes.

1.2 Setting

Research on product market decisions, such as product mix or quality decisions, requires granular information for which traditional financial disclosures offer poor guidance. Existing studies on product quality for instance focus on specific sectors where

[7]For instance, an alternative explanation can be that some supermarkets are included in vertically-integrated distribution chains because they have weak ability to negotiate with suppliers. If those negotiation skills (or bargaining power) also affects the relation with creditors, this could drive the observed effect.

this information is available outside companies (Matsa, 2011; Phillips and Sertsios, 2013). This information is usually compiled by industry authorities or business watchers. In this paper, I use the wine industry as a laboratory to test my research question. Wine making is a very regulated activity in the majority of the European wine producing countries. Companies are required to disclose detailed information on production and inventory levels to competent authorities, which use this information to create exhaustive industry records that support policy and regulatory decisions.

Wine industry provides a suitable setting to explore the effect of financial constraints on product mix decisions. First and most importantly, product mix decisions can be accurately observed. Given the importance of this first requirement, I devote the following subsection to describe it in detail. Also, there is a close link between product choice and the length of the production cycle. On average, higher quality products are associated with longer ageing periods (Jackson, 2008, page 441). Although there may be some exceptions, a positive relation holds in the aggregate.[8] In some wine producing countries, there is even specific regulation imposing minimum ageing periods for several wine categories and usually for higher quality tiers. Finally, the wine making process requires wineries to hold inventories over long periods (usually longer than one year), and production occurs once and at a specific time of the year. These features are important as they allow the matching between each production cycle (starting with harvest and grape processing) and the frequency of standard financial data.[9]

[8]To establish my argument, ageing does not necessarily need to be a driver of quality. Although this is certainly true for some wines, a positive correlation between the two suffices. That being said, one can think of alternative, unrelated to quality, explanations to why better wines may take longer to be placed on the market. Some examples include longer certification processes or different demand for wines in different tiers.

[9]Lovell (1961) mentions that "The task of investigating dynamic inventory phenomena is complicated by the difficulties involved in obtaining appropriate data based on observations collected at more frequent than yearly intervals. Since the planning horizon of the firm is surely shorter than a year (...), annual data will not do."

1.2.1 Product Mix Measurement

I make use of the standardized classification scheme for agricultural products and foodstuffs introduced by European Union in 2012.[10] With the goal of promoting and protecting the reputation of regional products, the *Protected Designation of Origin* (PDO) and *Protected Geographical Indication* (PGI) denominations soon started appearing in the labels of several agricultural products such as wine, cheese, fruit, etc. Concerning wine production, certification systems and information disclosures regarding production characteristics have long been a practice in relevant wine-producing countries. Therefore, this new classification system was similar to the traditional *appellation* systems in place for decades in those countries, which legally defined and protected geographical indications (Jackson, 2008). Due to this fact, the classification resulting from national transpositions of the European regulation overlapped with the traditional classification in countries such as France, Italy and Portugal.[11]

In Portugal, wines certified with any of these mentions are perceived by the market as different products. Besides avoiding the misleading of consumers by non-genuine products, PDO and PGI denominations are associated with higher quality standards due to more rigorous production methods and certification processes. PDO is the highest quality category of wines followed by PGI.[12] The European Union certifies as PDO those products that are 'produced, processed and prepared in a given geographical area, using recognized know-how (...), whose characteristics are linked to their geo-

[10]EU Regulation No 1151/2012 of the European Parliament and of the Council of 21 November 2012

[11]In France, for instance, the traditional Appellation D'Origine Controlée works in parallel with the new PDO nomenclature (Appellation D'Origine Protégée), but usually only the traditional one is presented in labels. The matching between these two classification schemes allows to apply the same classification standards for wines produced before 2012 in Portugal. Due to this fact, I will employ PDO and PGI nomenclature for all years in the sample.

[12]In some countries, there are some few examples of PGI wines whose reputation and price exceed those of PDO counterparts. I ignore those marginal cases as I am not able to identify them. If anything, this omission shall attenuate the findings.

graphical origin. They must adhere to a precise set of specifications (...).' In the case of wine, the International Organization of Vine and Wine adds that this nomenclature is linked to a quality level attributed to the geographical milieu including natural or human factors.[13] The PGI denomination works similarly to PDO, although production rules are no so stringent as those applied to PDO. These two categories contrast with a third-category - non-classified wine.[14] Wines in this category are not subject to specific regulation or rigorous certification processes. Based on these three categories, consumers are able to establish a ranking of wine quality. Figure 1.2 exhibits average prices for each wine category in 2016 in some relevant European wine-producing countries. For example, in Portugal the average price per litre of PDO wine was roughly EUR 3, higher than PGI (EUR 2.43) and non-classified wine (EUR 1). Average prices for each of these categories in different European geographies do indeed confirm that the market perceives each product differently. I use this categorical distinction as a metric of product mix. In the European market, all the three categories have a significant expression. In 2016, total production was composed by 43% PDO, 21% PGI and 27% non-classified wines (ISMEA, 2017).[15]

A caveat of this approach it that it does not allow to distinguish wines within each classification. This feature marks a departure from recent literature on wine value, which uses wine-specific metrics such as experts' ratings or auction outcomes (Gibbs et al., 2009; Dimson et al., 2015). Unlike my approach, these methodologies usually focus on wines produced by famous winemakers and do not cover the entire spectrum of companies.

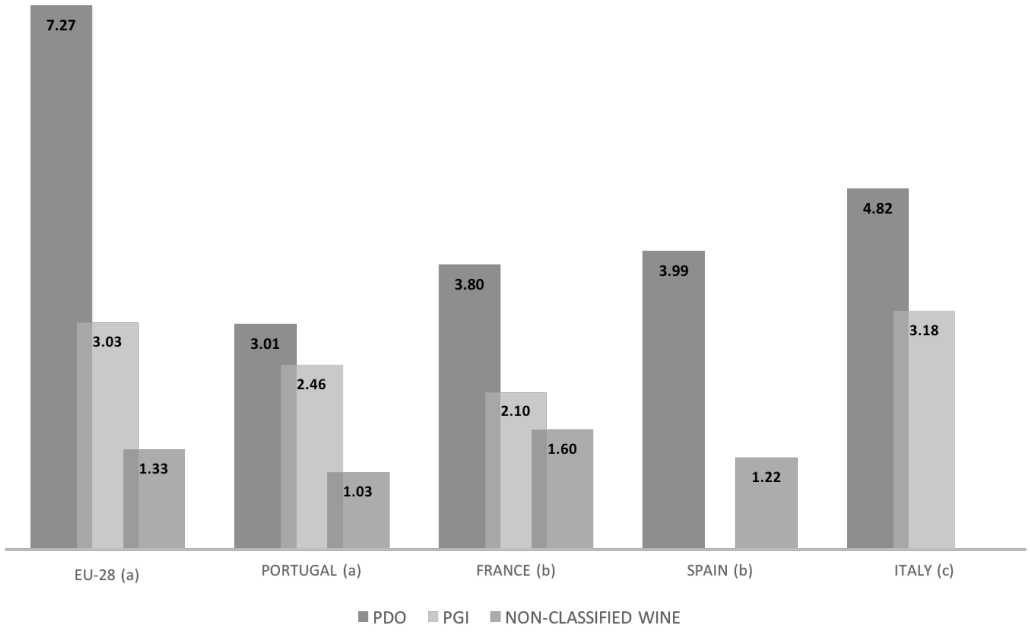
[13]International Standard For The Labelling Of Wines, OIV.

[14]In some countries, this category is also called *Table Wine*. I opted not to use this term as it is not consistent across countries.

[15]The remaining 9% is must or varietal wine. The source distinguishes these two from the three main categories.

Figure 1.2: Average wine price (EUR/litre) in 2016, by product category

This figure displays the average wine prices (EUR/litre) by product category for major European wine producing countries. The product categories are: Protected Denomination of Origin (PDO), Protected Geographical Indication (PGI), and Non-Classified Wine. Export prices, domestic consumer prices and price on large distributors are presented for regions marked with (a), (b) and (c), respectively. All figures are reported as of 2016. *Sources*: Eurostat (EU-28), Instituto da Vinha e do Vinho (Portugal), Statista.com (France and Italy) and Observatorio Español del Mercado del Vino (Spain).



1.2.2 Wine production in Portugal

Wine making has a long tradition in Portugal and wine has long been one of the Portuguese most exported products. According to the International Organization of Vine and Wine, Portugal ranked second in surface devoted to vines as a percentage of total country surface (2,1%) in 2016, only surpassed by Italy (2,3%). This importance extends to international trade, where Portuguese wines accounted for 2,7% of total wine exports in the world, ranking 9th in this list. A historical driver of this international success was an early regulation and monitoring of the sector. The first wine regulation dates back to 1756, with the establishment of the Douro Demarcated Region (where

the famous Port wine is produced and exported from). This fact makes that region one of the oldest demarcated regions in the world. The idea of sector supervision persisted strongly until today and extends now to grape and wine production in all wine regions. A visible dimension of such supervision is the maintenance of *current accounts*. This is an exhaustive registration encompassing harvest, production and stock declared by wine makers to sector regulators on an annual basis. In order to accomplish an effective supervision, the Portuguese territory is divided into fourteen wine regions (figure A1). Each region has specific regulation on production processes, quality control and certification, and is supervised by an autonomous regional regulator. Besides ensuring companies comply with the regional-specific regulation, the regional regulators control and issue PDO and PGI denominations.

Figure A3 depicts the main stages of the wine production process. Wineries can produce grapes in their own estates as well as acquire grapes from other winegrowers. In Portugal, the latter is very common and allows producers to reach larger production volumes. The vines where grapes are harvested from may be classified as suitable for PDO or PGI (*appellation system*). Later on, producers will be able to request any of these certifications only for the quantity of wine made out of those grapes. This is a necessary, but far from sufficient, requirement for certification. Due to the *appellation system*, wineries commit to a provisional "maximum-quality" annual product mix in the beginning of each production cycle with the choice of the grapes used.[16] They are not bound to their vineyards though. In the case they own vineyards suitable for a given classification but prefer a different product mix, they might acquire grapes with a different or with no classification, selling their own if necessary. The next stage, shortly after harvest and grape acquisition, is grape processing into wine. By the end of this stage, companies are required to report production levels by type, suitability for any

[16]Declassification is possible but subject to regulators' approval.

certification and color. The *current accounts* are updated with this new information.

Then, ageing can last for long periods depending on the type of wine. As soon as the wine is ready to be introduced in the market, wineries may request certification. The certification process encompasses three consecutive stages: administrative registration, physico-chemical and sensory analysis, and labeling. Administrative registration encompasses cross-validation of the quantity requested for certification and the one previously declared in the *current accounts*. In the second stage, winemakers deliver wine samples at accredited laboratories where chemical analyses take place. In parallel to this process, an accredited tasting panel carries out a sensory trial. Conditional on the results, the regulator then decide whether a wine fulfills the requirements to be certified as PDO or PGI. If a wine is approved with any of the denominations, the regulators are also responsible for verifying that the information in the label complies with European, national and regional guidelines and that comprehensive product information is made available to consumers. The approval of the label translates into each bottle receiving a numerically traceable seal of guarantee. This seal attests quality and origin of the product to the consumers. Figure A2 illustrates PDO and IGP seals of guarantee in Alentejo region. Wines that do not fulfill the requirements to be awarded one of the two quality denominations can still be traded as non-classified wine. Table A1 presents a more detailed description of each production stage.

Wine production is also divided into wine types. In Portugal there are three main types: still, liqueur and sparkling wine. PDO and PGI denominations can be attributed to more than one type in some regions, while in others only still wine can be granted one of these denominations. Table A2 summarizes the certified categories by region.

Another particular difference across regions is the existence of region-specific regulation imposing minimum ageing periods. Minimum ageing periods are mandatory by law and

regional regulators often carry out on-site inspections to production facilities and wine cellars.[17] The required ageing periods differ across regions, type and color (red or white). In table A2, I present the mandatory minimum ageing period (in months) imposed by each regional-specific regulation. A company in Alentejo for instance can produce PDO, PGI or non-classified still wine with no mandatory ageing period. In contrast, a company in Algarve producing PGI or non-classified still wine is not subject to any ageing restriction but faces a 6-month minimum ageing requirement on PDO red wine production. For a large majority of regions where these regulations are in place, they only apply to PDO wines (the only exceptions are Douro and Azores). Color is also a distinctive feature in what concerns minimum ageing.[18] Looking at the columns that split still wine into red and white types, minimum ageing regulations apply only to red wines for a majority of the regions. In the case they apply to both red and white, red wines tend to have a longer minimum requirement than white wines.

The wine industry in Portugal is populated by micro, small and medium-size companies. A large majority exhibits sole-proprietorship or private limited ownership structures and relies on banks as the primary source of external capital. By focusing on this setting, I ultimately depart from the existing literature that focus on large or public companies.

[17] Imposing minimum ageing periods is not the only way to ensure an appropriate ageing period. In the *Port Wine* case, existing regulation imposes a sales limit on young wines (Viana and Rodrigues, 2006).

[18] Color (red, white or rosé) is a primordial division among still wines (Jackson, 2008, page 8). In Portugal, rosé wine does not have a significant expression when compared to the other two. Therefore, I treat rosé wine as white given the similarities regarding the production process.

1.3 Data and Empirical Methodology

1.3.1 Data Sources

I obtained access to the Portuguese *domestic wine file* from *Instituto da Vinha e do Vinho* (IVV). IVV is the national regulatory agency for the wine sector under the Portuguese Ministry of Agriculture. It coordinates the regional regulators responsible for the control and certification of wines and is responsible for maintaining and developing the *domestic wine file* (*current accounts*). This file is an exhaustive annual dataset comprising information spanning the wine production cycle: harvest, production and stock.[19] Winegrowers and winemakers are required to report production levels (Harvest-Production Declaration) and inventory levels (Inventory Declaration) in each season in accordance with Portuguese regulation.[20] The information collected in these two fillings is the primary input of the *domestic wine file*. This dataset is not publicly available and only sector aggregate statistics are published.

The data comprises all Harvest-Production and Inventory Declarations submitted by wineries between 2006 and 2016. In the Harvest-Production Declaration, companies report grape quantities by product type, color, region and destination (own production, delivery at a cooperative winery or sale to other winemaker) as well as the production volume of new wine by product type and color. Inventory Declaration is also a very detailed form, where companies report stock characteristics, namely quantities by location, product type, color, year of origin, and in-bulk/bottled status.

I supplement the main dataset with the Central Balance Sheet and Central Credit Re-

[19]Only producers whose wine production does not exceed 4 000 litres and is destined for self-consumption are exempted from registration.

[20]Harvest-Production Declaration has to be submitted between the 1st of October and 15th of November, by the time harvest and grape transformation (into wine) must be completed. The Inventory Declarations are reported from the 1st of August to the 10th of September relative to inventory levels as of 31st of July.

gistry provided by *Bank of Portugal* (central bank).[21] The former is a comprehensive balance sheet dataset for Portuguese firms. The Credit Registry details information on firm-level aggregated loan amounts and characteristics, such as total, effective, potential and overdue credit amounts; corresponding breakdown by maturity, collateral and guarantees required; and number of bank relationships. It aggregates all credit relationships between financial institutions and financial publicly limited or limited liability companies. This information is assembled by the central bank from commercial banks and other credit institutions on a monthly basis.[22]

I focus on companies for which financial data is available. I eliminate firm-year observations with negative total assets, total liabilities or cash. I also remove observations when cash or inventory are higher than total assets or when the company reports less than two employees. In order to alleviate concerns that companies may not invest in higher quality products because they lack the necessary knowledge or technology, I condition the analysis on companies that report a positive production of PDO wine at any point in time. Finally, I keep companies for which I have at least 3 years of data and operate in only one region. All variables are winsorized at the 1 and 99 percentiles.

1.3.2 Empirical Methodology

The identification strategy exploits an unexpected shock to firms' credit availability - the European Banking Authority's 2011 Capital Exercise. Aiming to strengthen European banks' capital buffers against sovereign debt exposures, EBA announced a mandatory increase in capital requirements in October 2011. The measure established individual banks had to raise core tier 1 capital ratios (CT1) to 9 percent of their risk weighted

[21] Other papers using CB and CRC include Iyer et al. (2013) and Blattner et al. (2018).

[22] This dataset includes all loans above 50.€ Such a low reporting threshold rules out the possibility of under-reporting for smaller companies.

assets (RWA).[23] It targeted the major banks in each European country.

This regulation was largely unanticipated by economic agents (Gropp et al., 2018; Blattner et al., 2018). In that year, EBA had already conducted relevant stress tests and by that time, it had already disclosed information on the exposure of each European bank to sovereign risk.[24] Furthermore, there was no clear sign that a rise in capital requirements would induce a credit supply shortage. In fact, EBA strongly recommended banks to address capital shortfalls without reducing credit supply to the real economy. Given the unpredictability of this exercise and of its potential impact on the corporate sector, it is unlikely that individual companies anticipated a contraction in credit availability. Another distinctive feature of this quasi-natural experiment is the fact that EBA Capital Exercise targeted only the largest banks in each country, creating a well-defined control group. Finally, the Capital exercise deadline was set at June 2012. This exceptionally short time period (8 months) make more plausible that any observed impact is a consequence of the capital requirement shock.

Besides all efforts to ensure that the capital ratio was not 'achieved through excessive deleveraging, disrupting lending into the real economy', there is evidence that banks restricted credit supply. Using data at the European level, Mésonnier and Monks (2014) show that a bank that had to increase its capital ratio by 1 percent of RWA had on average an annualized loan growth 1.2 percentage points lower than unaffected banks. Similarly, Gropp et al. (2018) document that targeted banks increased capital ratios by 1.9 percentage points compared to the unaffected banks, and this increase was achieved mainly by reducing risk-weighted assets (denominator) than by increasing levels of capital (numerator). The decrease in RWA occurred mainly through a contraction of

[23]This new level of core-tier-1-to-RWA ratio was not related to the risk exposure of any particular bank and represented a sizable increase relative to the previous level established under Basel III. More information about the EBA Capital Exercise can be found at: https://eba.europa.eu/risk-analysis-and-data/eu-capital-exercise

[24]None of the banks that failed the stress test were targeted by the Capital Exercise.

outstanding loans. They examine syndicated loan data in order to disentangle shifts in supply and demand for credit and find that affected banks reduced their credit supply by 27 percentage points relative to the remaining banks. In Portugal, there is evidence of a similar effect, where banks responded to higher capital requirements by restricting lending (Blattner et al., 2018).[25]

I obtained information about credit relationships with affected banks at the time of the announcement from *Bank of Portugal*, specifically the number of relationships with affected banks and the share of credit granted from those banks.[26] In table A3, I present difference-in-differences estimates of the impact of EBA Capital Exercise on firms' total credit. In the first two columns, I present estimates on the full sample. An increase in the share of credit from affected banks by one standard deviation is associated with a 5% decrease in total companies' debt in the years after the shock. Next, I condition the analysis on the sample of bank dependent companies (columns 3 and 4). The intuition is that the effect of the shock is expected to be higher for bank-dependent companies, i.e. companies whose bank loans represent a larger share of total financing. I define as bank dependent all companies that have a high debt ratio (non-current liabilities scaled by total assets above the median in the end of 2011). As expected, the impact on total credit outstanding is stronger among this sub-sample. A standard deviation increase in the share of credit from affected banks reduces total credit outstanding by 11%. In the last two columns, I show the estimates of a triple difference specification on the full sample. I find that the negative impact of the EBA Capital Exercise is driven by the cohort of companies that have higher share of credit from affected banks but are also bank dependent. Given these results as well as the evidence from existing literature,

[25]In Portugal four banks have been affected by the EBA Capital Exercise. Those are: Caixa Geral de Depositos, SA; Banco Comercial Portugues, SA; Espirito Santo Financial Group, SA; and Banco BPI, SA.

[26]Due to confidentiality, the identity of creditors was not made available.

I exploit the EBA Capital Exercise as a quasi-natural experiment. Importantly, I do not regard this shock as a pure reduction in leverage, which could theoretically lead to higher debt capacity in the future. Instead, the EBA Capital Exercise adversely impacted on credit availability in the short- and medium-term.[27]

I estimate the following difference-in-differences specification:

$$\begin{aligned}\text{Product Mix}_{ijt} = \beta_1 \text{Post}_t + \beta_2 \text{Post}_t \times \text{EBA Share}_i + \beta_3 \text{Post}_t \times \text{Bank Dep.}_i \\ + \theta \text{Post}_t \times \text{EBA Share}_i \times \text{Bank Dep.}_i + \gamma X_{it} + \delta_i + \delta_t + \delta_{jt} + \epsilon_{ijt} \quad (1.1)\end{aligned}$$

where Product Mix_{ijt} is the share of PDO wine of company i, in wine region j, in year t on company's total production. Post_t is a binary variable equal to 1 after the EBA Capital Exercise (2012 onward). The variable EBA Share_i is the share of credit from affected banks at the time of the announcement (October 2011). Bank Dep._i is a binary variable taking on the value of 1 if the company had a high debt ratio in the end of 2011 (non-current liabilities scaled by total assets above the median). In addition, the specification includes one-year lagged company's characteristics such as Size_{t-1} (logarithm of total assets), Internal Market_{t-1} (percentage of sales in the domestic market), and Any PDO_{t-1} (a binary variable indicating whether the company has produced any PDO wine in the previous year). I include firm-cluster fixed effects δ_i which absorb all cluster-specific credit demand shocks (Gropp et al., 2018) and year-region fixed-effects δ_{jt}. The latter are intended to control for specific year-region characteristics, such as climate variation and shocks in credit supply or in consumer preferences. All standard errors are clustered at firm level.[28]

[27]This distinction is particularly important in a setting composed by small and medium size companies, where identifying financial constraints is difficult.

[28]Preferentially, I would like to cluster at creditor level as this is the main dimension of exposure

The coefficient of interest θ measures the impact of an exogenous increase in the likelihood of a credit contraction (as a result of exposure to EBA affected banks) on product mix. The triple difference specification is motivated by the findings in table A3. This approach allows to compare bank dependent companies with high exposure to affected banks (*treatment group*) with companies that are not exposed to affected banks or not dependent on bank credit (*control group*). Amiti and Weinstein (2018) take a similar approach and note that "a given bank shock is likely to have a much larger impact on the investment rate of a firm that finances, say, 80 percent of its capital through bank loans than on a firm that finances only 1 percent of its capital from loans". Alternatively, I estimate the specification as a double-difference conditioned on bank dependent companies.

In order to evaluate whether the length of the production cycle plays a role in product mix decisions, I run the previous specification for groups of companies with *ex-ante* different production cycle duration. In the main analysis, I split companies according to regions where a mandatory minimum ageing regulation is in place (see section 1.2.2). This regulation imposes a minimum ageing threshold on PDO wines. If the length of the production process indeed constrains companies in their product mix decisions, I expect this group of companies to exhibit a different behavior following the credit supply shock. Yet, it is difficult to rule out other potential effects that might have occurred at the same time and might have impacted these groups differently (e.g. different credit supply shifts, changes in input or consumer prices, changes in consumer preferences). In order to alleviate this concern, I include year-region fixed effects in the specification. This attenuates the concern if those effects are likely to have taken place at region level. Furthermore, I go one step further and analyze within-company product mix decisions. As the minimum ageing regulations apply mainly to red wines, I analyze whether the

to the shock. Unfortunately, I do not have information on each company's list of creditors.

adjustment is stronger for this type of wine. If the production cycle mechanism is at work, companies may be likely to adjust preferentially the production of red wine.

1.3.3 Descriptive Statistics

In panel A of table 1.1, I present the number of observations. I obtained production data for 52 191 companies (278 371 firm year observations). Many of these producers operate as individual producers and are not formally registered as a company (financial data is not available). In effect, I have production data for 3 148 producers registered as companies (e.g. private limited), and 574 firms with available financial data (4 235 firm-year observations). Although I end up with a small fraction of all producers, they represent a sizable share of total wine production. The companies in the final sample represented around 67 percent of total production in 2008. I present a similar analysis for data on harvest and stock. By looking at a representative sample of domestic wine producers, I distinguish from other papers that focus on wines from famous wineries (Gibbs et al., 2009; Dimson et al., 2015). In panel B, I present descriptive statistics for companies in the final sample. The average size (total assets) is EUR 4.13 Million and have on average 19 employees. The typical firm in the final sample is mature (25.9 years). These companies hold high levels of inventory (24% of total assets), particularly in the form of finished inventories (21%). Inventories are not only high in volume but also kept for long periods. The days-sales-of-inventory ratio shows that on average these companies take 1 397 days to convert its inventory into sales. This distribution is skewed to the right though, with several companies with very long days in inventories. Although shorter, the median is still 552 days in inventories.

I present some descriptive statistics regarding the EBA Capital Exercise in panel C. At the time of the announcement (October 2011), companies in the sample had on

Table 1.1: Number of observations and firm descriptive statistics

Panel A of this table displays the number of company-year observations for different samples. "Formally Registered" refers to the sub-sample of entities (vintners, winemakers or bottlers) formally registered as a company. The last column exhibits the share in volume (harvest, production or in-stock wine) of companies in final sample relative to the aggregate total volume. These values are computed as of 2008. Panel B presents financial characteristics of companies in final sample. Panel C presents summary statistics of bank relationships at the time of EBA Capital Exercise announcement in October 2011 (one observation per company). The sample period is 2006-2016.

Panel A: Number of observations

	All Data	Formally Registered	Final sample	% Total volume (2008)
Harvest	374 990	21 137	3 094	18%
Production	278 371	15 315	4 235	67%
Stock	32 958	11 589	3 870	58%

Panel B: Firm Characteristics

	Obs.	Mean	St. Dev.	25%	50%	75%
Total Assets	5 031	4 129 119	5 204 988	581 512	1 864 778	5 282 216
Cash	5 010	118 850	220 780	8 092	28 585	105 749
Total liabilities	5 031	2 404 748	3 145 739	319 093	1 002 835	3 135 860
Sales	5 017	1 612 749	2 699 776	124 738	406 843	1 574 502
Net income	5 031	10 013	168 278	-34 519	3 908	34 480
ROA	4 556	-0.01	0.16	-0.03	0.00	0.02
Leverage	5 031	0.69	0.60	0.47	0.64	0.84
Inventory	5 031	819 329	974 276	77 882	343 483	1 268 877
Inv./Assets	5 031	0.24	0.18	0.10	0.20	0.35
Days in inv.	4 911	1396.9	2232.7	223.9	552.7	1422.1
Nr. Employees	5 031	18.7	41.4	4.0	8.0	19.0
Firm age	5 028	25.9	23.9	9.0	18.0	38.0
Pct. Domestic Market	5 017	0.88	0.21	0.85	0.99	1.00
Bank Rel.	4 787	2.8	2.6	1.0	2.0	4.0
Largest bank rel.	4 350	0.77	0.24	0.57	0.84	1.00
Pct. short-term	2 932	0.49	0.38	0.10	0.47	0.99
Personal Guarantee	2 966	0.47	0.41	0.00	0.40	0.96

Panel C: EBA capital exercise (Oct 2011)

	Obs.	Mean	St. Dev.	25%	50%	75%
No. bank relationships:						
All	439	3.2	2.6	1.0	2.0	4.0
With affected banks	439	1.2	1.0	1.0	1.0	2.0
With affected banks:						
At least 1 relation	439	0.75	0.43	1.00	1.00	1.00
Share of credit	439	0.49	0.41	0.00	0.51	0.94

average credit relationships with 3.2 banks and 75 percent of the companies had credit relationships with at least one of the affected banks.[29] The average share of credit from affected banks is 49%.

Next, I present some descriptive statistics on wine production (table 1.2). PDO wine corresponds to 60 percent of total production on average. The remaining categories

[29]The high number of bank relationships with affected banks reflects the sizable market share of those banks. Recall that the EBA Capital Exercise targeted specifically the largest banks in each country.

Table 1.2: Wine production descriptive statistics

This table reports summary statistics related to wine production. In the last two rows, statistics are computed taking one observation per company. All statistics are presented for the final sample.

	Obs.	**Mean**	**St. Dev.**	**25%**	**50%**	**75%**
Harvest for own production	3 094	0.82	0.33	0.78	1.00	1.00
Production by denomination:						
PDO	4 235	0.60	0.40	0.15	0.75	1.00
N. Class.	4 235	0.14	0.28	0.00	0.00	0.10
Production by type:						
Still	4 235	0.95	0.16	1.00	1.00	1.00
Liqueur	4 235	0.03	0.13	0.00	0.00	0.00
Red wine	4 235	0.58	0.33	0.27	0.68	0.85
Bottled wine	3 870	0.22	0.24	0.05	0.13	0.31
Region Min. Ageing Restriction	545	0.44	0.50	0.00	0.00	1.00
Producer-bottler	554	0.96	0.20	1.00	1.00	1.00

(PGI and non-classified wine) have both significant expression as well. Still wine is the main type of wine produced (95%).[30] The average company produces 58 percent of red wine in each production cycle. Given the size of companies in the final sample, it is composed predominantly by producer-bottlers (96%), i.e. companies that control the production process from the wine-making phase until commercialization. Roughly half of the companies operate in regions with minimum ageing regulations (44%).

In panel A of table A4, I present a means comparison between the sub-sample of firms with at least 50% and the sub-sample of companies with less than 50% of credit from affected banks. The former sub-sample is composed by larger companies (according to total assets, number of employees and sales). Apart from size, these groups do not differ regarding other financial characteristics or wine production characteristics (the only exception is the production of red wine). Naturally, the sub-sample of companies with more than 50% of credit from affected banks differs from the other group in terms of bank relationship characteristics.

Panel B of table A4 exhibits a comparison of average characteristics of companies in

[30]This figure may be slightly inflated due to missing observations for companies in Douro region. This is the region with the most significant production of liqueur wine (*Port wine*), both in terms of value and volume.

regions with and without minimum ageing regulations. Companies in these regions do not differ in terms of financial characteristics. The only exception is the days sales of inventory ratio. Unsurprisingly, companies in regions where an ageing restriction is in place hold inventories over longer periods. This difference is statistically significant at 10% significance level. Companies in these regions differ in terms of wine production characteristics though. Specifically, companies in regions with minimum ageing restriction produce a lower percentage of PDO wine and more red wine. Importantly, they do not differ in the incidence of the shock. There is no statistically significant difference regarding the number of relationships with or the share of credit from affected banks. This fact arises from the country-wide presence of the targeted banks.

1.4 Results

This section presents the results. I start by showing evidence in favor of the parallel trend assumption in the context of the quasi-natural experiment induced by the EBA Capital Exercise. I then present estimates on the effect of the EBA Capital Exercise on product mix decisions. In order to assess the relevance of the production cycle mechanism, I perform the analysis on groups of companies with *ex-ante* different production timings. In particular, I exploit the regional variation in the existence of minimum ageing regulations. I further explore this mechanism by analyzing within-company decisions, namely the production of PDO red and white wine.

I test whether adjusting production (which will be converted into cash-flow some months or years later) is a natural response to a contemporaneous credit shock. Under financial constraints, companies can alleviate those constraints by selling current inventories immediately (e.g. inventory fire sales). I investigate if the effect is stronger for the set of companies with lower stock, i.e. those for which disposing of inventory may not be

so effective (or insufficient) in relaxing financial constraints. By providing an answer to this question, I investigate potential complementarity between production decisions and current stock characteristics.

Finally, I investigate whether adjustments in product mix are an efficient response to credit constraints.

1.4.1 Credit constraints and Product Mix Decisions

The quasi-natural experiment induced by the EBA Capital Exercise allows to explore the response of companies when faced with an exogenous contraction in credit supply. I have shown in table A4 that more exposed (treatment) and less exposed (control) companies do not differ in terms of financial characteristics, except in size. Due to this fact, I control for size in all specifications. However, the internal validity in a difference-in-differences setting requires a (pre-shock) parallel trend. In figure 1.3, I present evidence in favor of the parallel trend assumption. I plot the coefficients and corresponding confidence intervals of interaction regressors between all year-dummies and a treatment binary variable.[31] I observe that the two groups of companies do not differ in terms of PDO production before the shock, but present a different behavior right after (2012 and 2013).[32] Such evidence, coupled with the very narrow adjustment period imposed by EBA, makes me confident about the internal validity of this quasi-natural experiment. Moreover, the fact that affected banks had country-wide operations creates room for within-region identification as I observe affected (treatment) and unaffected (control) companies operating in the same region. This alleviates the concern that other variables taking place at region level could drive a different reac-

[31]The treatment variable assumes the value of one if a company's share of credit from affected banks is higher than 50% and is bank dependent.

[32]In figure A4 in the appendix, I plot the median and mean PDO wine production over time. It also shows a similar evolution between treatment and control groups before the shock.

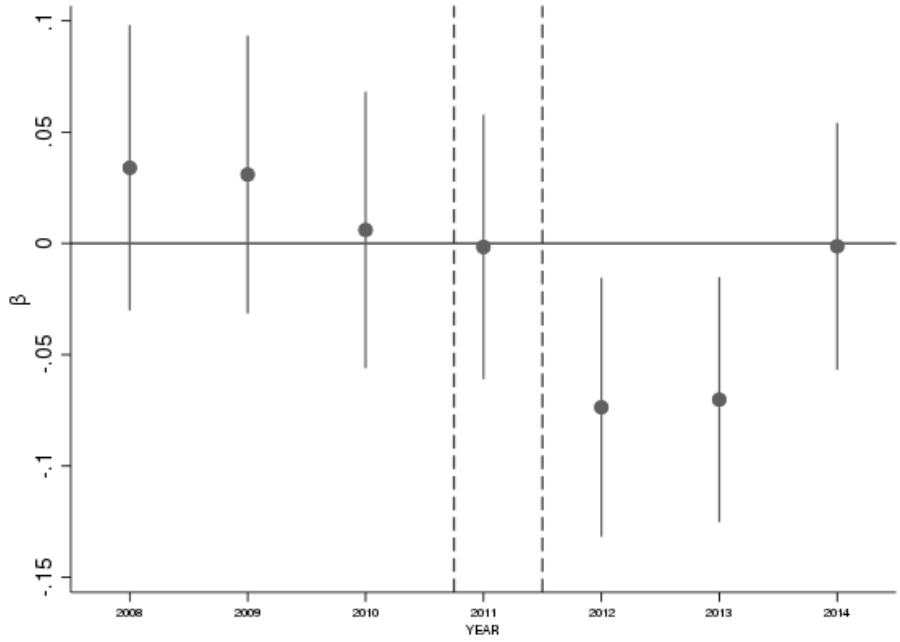
tion between the two groups. Some examples include changes in input prices, shifts in consumer preferences, or even differential impacts of the 2008 financial crisis across regions.

In table 1.3, I present estimates of the effect of credit constraints on product mix decisions. The main outcome is the share of PDO wine on company's total annual production. The econometric specification follows equation 1.1. I find a sizable and statistically significant reduction in PDO wine production as a response to the credit contraction induced by the EBA Capital Exercise. An increase in EBA Share of credit by one standard deviation reduces the percentage of PDO in total product mix by 5 to 7 percentage points among the group of bank dependent firms. This is the group that has been significantly affected by the shock as seen in table A3. The result is robust to the inclusion of firm fixed and year-region fixed effects and is statistically significant at least at 5% significance level in all specifications. I also observe that there is a high persistence of PDO production across time. The variable Any PDO, which equals one whenever a company has produced PDO wine in the previous year, is a strong determinant of PDO production in the following year. Yet, the effect of the shock is robust to the inclusion of this variable. In table A5 in the appendix, I present similar results using a double difference specification. This analysis is conditioned on the set of bank dependent companies. The findings are similar, although the statistical significance is affected by the reduction in the number of observations.

A potential concern with the interpretation of the previous results arises from the fact that the 'Bank Dependent' variable may be capturing other financial characteristics. To address this issue, I extend the main specification with other financial characteristics of interest. Specifically, I look at size, debt maturity and trade credit. Results are shown in table 1.4. The adverse effect of the shock is stronger for large companies and companies with a higher percentage of short term credit. Conversely, reliance on

Figure 1.3: Parallel trend

This figure plots the coefficients and confidence intervals (95% significance level) of the effect of the EBA Capital Exercise on PDO certified wine production. The dependent variable is the percentage of PDO wine in total wine produced. The explanatory variables are annual dummy variables that take the value of one in year t if a given company has been affected by the EBA Capital Exercise. I define as affected a company that had at least 50% of total credit from affected banks and is bank dependent. The regression controls for logarithm of size and includes year fixed effects and robust standard errors. Vertical lines delimit the period between EBA Capital Exercise announcement and deadline.



trade credit (measured by accounts payable) as an alternative source of financing attenuates the adverse impact of the credit contraction. Importantly, the main coefficient remains unaltered after the inclusion of these variables. This is evidence that the 'Bank Dependent' variable is unlikely to be capturing other potential confounding financial characteristics.

In the face of credit constraints, there are other margins in which companies in our sample could adjust, such as total quantity produced or the percentage of grapes sold right after harvest.[33] In table 1.5, I investigate whether companies have adjusted in any of these margins as a response to the shock. Looking at the log of quantities, I

[33]Another interesting margin is the adjustment in sale prices. Unfortunately, I do not have information on prices by company.

Table 1.3: Effect of Credit Constraints on Product Mix Decisions

This table presents the impact of the EBA Capital Exercise on product mix decision. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size$_{t-1}$ is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.136**	-0.144***	-0.166***	-0.133**	-0.132**	-0.151***
	[0.066]	[0.054]	[0.055]	[0.060]	[0.052]	[0.052]
Post × EBA Share	0.035	0.046	0.062**	0.048	0.044	0.052*
	[0.037]	[0.028]	[0.031]	[0.033]	[0.027]	[0.030]
EBA Share × Bank Dep.	-0.028			0.086		
	[0.101]			[0.085]		
Post × Bank Dep.	0.028	0.052	0.064*	0.063	0.058	0.064*
	[0.043]	[0.038]	[0.039]	[0.041]	[0.038]	[0.038]
Post	0.012	-0.013	0.026	-0.012	-0.016	0.039
	[0.026]	[0.019]	[0.031]	[0.022]	[0.019]	[0.070]
EBA Share	0.020			-0.018		
	[0.068]			[0.058]		
Bank Dep.	0.057			-0.019		
	[0.061]			[0.053]		
Size $_{t-1}$				-0.039***	-0.018	-0.008
				[0.011]	[0.023]	[0.023]
Internal Market $_{t-1}$				-0.022	0.038	0.033
				[0.079]	[0.044]	[0.046]
Any PDO $_{t-1}$				0.531***	0.103***	0.106***
				[0.025]	[0.032]	[0.036]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	2471	2471	2320	1991	1991	1855
No. Firms		422	413		392	381
Adjusted R^2	0.002	0.77	0.773	0.225	0.795	0.797

do not observe any statistically significant difference between affected and unaffected companies.[34] Similarly, I do not find a differential response regarding the percentage of grapes kept for own production.

Overall, the results in this sub-section document an adverse impact of financial constraints on product market decisions of companies. This evidence is consistent with the empirical findings in Matsa (2011); Phillips and Sertsios (2013) and Kini et al. (2017).

[34]Looking at total quantity is also important because the main outcome variable is the percentage of PDO wine in total production. As I do not find any impact on quantity (denominator), the reported decrease in the percentage of PDO wine arises is due to a lower production of PDO wine (in levels).

Table 1.4: Is Bank Dependent Definition capturing other financial characteristics?

This table analyzes whether the Bank Dependent variable is capturing other relevant firms' characteristics. I extend the specification in table 1.3 with some interaction variables of interest, namely size (Large Company), debt maturity (High Short-Term Credit) and payment terms with suppliers (High Account Payable).The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Large company	*High Short-Term Credit*	*High Acc. Payable*
	(1)	(2)	(3)
Post × EBA Share × Bank Dep.	-0.188***	-0.157***	-0.153***
	[0.055]	[0.056]	[0.053]
Post × EBA Share	0.235***	0.118***	0.006
	[0.080]	[0.042]	[0.036]
Post × Bank Dep.	0.092**	0.053	0.065*
	[0.040]	[0.039]	[0.038]
Post	-0.074	0.008	0.044
	[0.090]	[0.073]	[0.069]
Post× EBA Share × *Large*	-0.201**		
	[0.079]		
Post× *Large*	0.118**		
	[0.057]		
Post× EBA Share × *High Short-Term*		-0.126**	
		[0.055]	
Post× *High Short-Term*		0.076*	
		[0.040]	
Post× EBA Share × *High Acc. Payable*			0.098*
			[0.051]
Post× *High Acc. Payable*			-0.034
			[0.037]
Size $_{t-1}$		-0.004	-0.008
		[0.024]	[0.023]
Internal Market $_{t-1}$	0.041	0.009	0.054
	[0.045]	[0.050]	[0.049]
Any PDO $_{t-1}$	0.103***	0.110***	0.107***
	[0.036]	[0.036]	[0.036]
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1855	1716	1808
No. Firms	381	352	371
Adjusted R^2	0.798	0.797	0.799

1.4.2 Production cycle mechanism

There are several mechanisms one may think of through which financial constraints may affect product market decisions. In this paper I study the production cycle mechanism. In the face of financial constraints, companies may be compelled to adjust product mix

Table 1.5: Effect of Credit Constraints on Quantity

This table presents the impact of the EBA Capital Exercise on quantity produced. From column 1 to 3, the outcome variable is the logarithm of total quantity produced. In columns 4 to 6, it is the percentage of grapes harvested kept for internal production. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size$_{t-1}$ is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Log(Quantity)			**% Harvest for Own Production**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.199	-0.014	0.041	-0.051	-0.014	-0.004
	[0.243]	[0.141]	[0.134]	[0.069]	[0.048]	[0.048]
Post × EBA Share	0.020	-0.053	-0.075	-0.033	-0.052*	-0.055*
	[0.156]	[0.084]	[0.082]	[0.043]	[0.029]	[0.028]
EBA Share × Bank Dep.	0.124			0.081		
	[0.469]			[0.077]		
Post × Bank Dep.	0.025	0.020	0.011	0.042	0.036	0.018
	[0.156]	[0.085]	[0.087]	[0.042]	[0.035]	[0.033]
Post	0.036	-0.394*	0.279***	0.005	0.733***	0.045
	[0.095]	[0.234]	[0.089]	[0.030]	[0.139]	[0.031]
EBA Share	0.391			0.057		
	[0.363]			[0.052]		
Bank Dep.	-1.041***			-0.050		
	[0.307]			[0.054]		
Size $_{t-1}$			0.183***			0.025
			[0.052]			[0.022]
Internal Market $_{t-1}$			-0.120			-0.035
			[0.113]			[0.036]
Any PDO $_{t-1}$			-0.003			-0.006
			[0.050]			[0.024]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	Yes	Yes	No	Yes	Yes
No. Observations	2471	2320	1855	1653	1513	1322
No. Firms		413	381		299	265
Adjusted R^2	0.04	0.915	0.929	0.012	0.713	0.645

as a means of shortening the production cycle. In order to isolate this mechanism, I exploit the existence of minimum ageing thresholds for some products. In particular, these restrictions apply mainly to PDO wine production. Exploiting this regional variation enables identifying the role of this mechanism on product market decisions.

In the first three columns of table 1.6, I estimate the main specification among the group of companies operating in regions where there is a minimum ageing restriction. I find a significant and strong reduction in the percentage of PDO wine. An increase in EBA Share by one standard deviation is associated with a reduction of 10 percentage points in

the production of PDO wine among bank dependent companies. In the following three columns, I analyze the impact of the shock in regions where there is no minimum ageing restrictions on the production of PDO wines. The point estimates are much smaller (and non-significant in two out the three specifications). In table A6, I show similar results following a double-different approach. The contrasting results among the two types of regions suggests that the ageing restriction (proxy for the length of production cycle) plays a role and constrains companies in their product market decisions.

The previous findings rely on the assumption that these regions do not differ in other dimensions beyond the existence of an ageing regulation. However, other variables such as production costs might have evolved differently after the shock among the two types of regions (e.g. cost of grapes acquired from winegrowers or certification costs). I try to mitigate this concern in several ways. First, the inclusion of year-region fixed effects shall attenuate this concern as it compares companies operating in the same region. Second, I can analyze within-company product decisions. Specifically, I propose to study the decision of producing PDO red or PDO white wines. In a large majority of regions where minimum ageing regulations are in place, they only apply to PDO red wines. If the crucial difference between these two types of regions is the existence of the ageing regulation, I expect to observe a stronger impact on the specific type of wine to which the regulation applies.

In table 1.7, I analyze the decision of producing PDO red and white wine among companies producing in regions where a minimum ageing restriction is in place. In the first three columns, the outcome variable is the percentage of PDO red wine. In the remaining columns, the outcome variable is the percentage of PDO white wine. I find that the effect reported in table 1.6 is entirely driven by an adjustment in the percentage of PDO red wine. There is no statistically significant shift on white wine production. Similar results are found using a double-difference specification (see table A7). By

Table 1.6: Effect of Credit Constraints on Product Mix Decisions by Regions with Minimum Ageing Restriction

This table presents the impact of the EBA Capital Exercise on product mix decision of companies inside or outside regions with minimum ageing restriction. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size$_{t-1}$ is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Regions with Minimum Ageing Restriction					
	Yes			**No**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.247**	-0.217***	-0.237***	-0.091	-0.098	-0.112*
	[0.102]	[0.079]	[0.080]	[0.085]	[0.066]	[0.067]
Post × EBA Share	0.034	0.036	0.047	0.059	0.059*	0.059
	[0.063]	[0.046]	[0.055]	[0.044]	[0.034]	[0.036]
EBA Share × Bank Dep.	-0.090			0.001		
	[0.142]			[0.135]		
Post × Bank Dep.	0.100	0.122**	0.136**	-0.008	0.025	0.029
	[0.068]	[0.060]	[0.063]	[0.055]	[0.046]	[0.046]
Post	0.020	-0.022	-0.068	0.001	-0.011	0.040
	[0.044]	[0.034]	[0.053]	[0.031]	[0.021]	[0.070]
EBA Share	0.171*			-0.061		
	[0.094]			[0.089]		
Bank Dep.	0.166*			-0.020		
	[0.084]			[0.077]		
Size $_{t-1}$		-0.045	-0.026		-0.013	0.001
		[0.041]	[0.041]		[0.027]	[0.028]
Internal Market $_{t-1}$		0.166**	0.134*		-0.042	-0.016
		[0.070]	[0.073]		[0.057]	[0.060]
Any PDO $_{t-1}$		0.054	0.054		0.146***	0.149***
		[0.041]	[0.048]		[0.045]	[0.049]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	1001	752	712	1404	1181	1143
No. Firms		168	168		215	213
Adjusted R^2	0.036	0.796	0.801	0.001	0.782	0.782

looking at this within-company margin of adjustment, I rule out other factors that could have potentially affected regions or even companies in a singular way. Therefore, this finding illustrates in a more clear-cut way the role of the production cycle mechanism.

In table A8, I present a similar analysis for regions where there is no minimum ageing restriction. In accordance with the findings in table 1.6, there is no statistically different response in the production of PDO red or white wine among this group of companies. Overall, affected companies adjust product mix as a response to financial constraints.

Table 1.7: Effect of Credit Constraints on Product Mix Decisions by Wine Color

This table presents the impact of the EBA Capital Exercise on product mix decision by wine color. The analysis is conditioned on companies operating in regions where a minimum ageing restriction is in place. The outcome variable is the percentage of red (first three columns) or white (last three columns) PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Color					
	Red			**White**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.175*	-0.149*	-0.219***	-0.072	-0.068	-0.018
	[0.098]	[0.089]	[0.071]	[0.046]	[0.049]	[0.029]
Post × EBA Share	0.001	0.027	0.041	0.033	0.009	0.006
	[0.055]	[0.039]	[0.044]	[0.028]	[0.017]	[0.018]
EBA Share × Bank Dep.	-0.104			0.014		
	[0.118]			[0.069]		
Post × Bank Dep.	0.084	0.090*	0.111**	0.016	0.032	0.025
	[0.061]	[0.053]	[0.054]	[0.025]	[0.024]	[0.022]
Post	0.016	-0.016	-0.060	0.004	-0.006	-0.008
	[0.038]	[0.026]	[0.044]	[0.016]	[0.012]	[0.016]
EBA Share	0.177**			-0.006		
	[0.075]			[0.036]		
Bank Dep.	0.159**			0.006		
	[0.072]			[0.043]		
Size $_{t-1}$		-0.047	-0.017		0.002	-0.009
		[0.037]	[0.037]		[0.015]	[0.017]
Internal Market $_{t-1}$		0.165**	0.103		0.001	0.031
		[0.076]	[0.065]		[0.048]	[0.029]
Any PDO $_{t-1}$		0.044	0.050		0.010	0.004
		[0.037]	[0.043]		[0.010]	[0.009]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	1001	752	712	1001	752	712
No. Firms		168	168		168	168
Adjusted R^2	0.045	0.759	0.788	-0.004	0.732	0.832

The previous evidence suggests that this adjustment occurs as a means of shortening production cycles.

1.4.3 Production Decisions and Characteristics of Current Stock

The previous results show that the credit supply shock induced by the EBA Capital Exercise had implications on product market decisions. Affected companies seem to respond to the shock by adjusting their product mix. Alternatively, a more direct way

to alleviate financial constraints and generate cash flow in the short- and medium-term would be an increase in sales (selling current inventory). This way, companies would eventually avoid unnecessary adjustments in production. Therefore, it is important to understand how adjusting current production (which only converts into cash flow some months or years later) can help ease financial constraints as opposed to selling inventory. In this section, I analyze whether there is a relation between adjustment in production and levels of inventory.

In table 1.8, I observe how the adjustment in product mix relates to levels of current stock. I split companies according to low (below median) and high (above median) inventories over total assets, either measured as balance sheet inventories (columns 1 and 2) or total wine in stock (columns 3 and 4).[35] The effect of the shock is very strong and statistically significant among the group of companies with low levels of inventory. Although the point estimates are negative among the other group as well, the effect is not statistically significant. These results show that the adjustment in production occurred precisely in companies with low levels of inventories i.e. the group of companies for which disposing of inventory may not be so effective in alleviating financial constraints. These results point to a complementarity between production and levels of current stock. As managers see lower inventory levels and eventually get closer to stock-out, they seem to start adjusting production in order to produce faster and replace inventories at a faster rate. In our setting, it translates into a change in product mix.

[35]Although positively correlated, the later measure differs from the former as it does not take into account the value of the inventories.

Table 1.8: Complementarity between Production Decisions and Characteristics of Current Inventory

This table presents the impact of the EBA Capital Exercise on product mix decision by levels of inventory. A company is defined as having a high level of inventory if it is above the median in the corresponding distribution. The median was computed on the entire sample, i.e. before dropping observations. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Inventories/Total Assets		**Stock/Total Assets**	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Post × EBA Share × Bank Dep.	-0.350***	-0.097	-0.218***	-0.052
	[0.118]	[0.065]	[0.078]	[0.073]
Post × EBA Share	0.096*	0.026	0.003	0.054
	[0.051]	[0.042]	[0.057]	[0.041]
Post × Bank Dep.	0.135***	0.035	0.135**	0.029
	[0.051]	[0.048]	[0.061]	[0.052]
Post	-0.036	0.086	0.041	0.109
	[0.406]	[0.057]	[0.124]	[0.073]
Size $_{t-1}$	-0.040	-0.019	-0.017	-0.017
	[0.048]	[0.034]	[0.038]	[0.029]
Internal Market $_{t-1}$	-0.029	0.088	-0.045	0.083*
	[0.083]	[0.062]	[0.080]	[0.049]
Any PDO $_{t-1}$	0.045	0.092**	0.135*	0.101**
	[0.060]	[0.041]	[0.072]	[0.048]
Firm FE	Yes	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes	Yes
No. Observations	453	1402	712	961
No. Firms	145	311	214	237
Adjusted R^2	0.72	0.825	0.765	0.863

1.4.4 Performance

In the previous sections, I document that companies adjust product mix as a response to credit constraints. In this section, I investigate whether this is an optimal response from the perspective of a company. I start by showing the direct impact of the EBA Capital Exercise. The EBA Capital Exercise induced an exogenous credit contraction. Thus, if anything, I expect to see a negative effect on performance outcomes of companies affected by this additional constraint relative to unconstrained companies. Then, it is also important to understand whether, among the set of affected companies, those that have adjusted their product mix indeed performed better than the ones whose

production remained unaltered. If anything, I expect companies that decreased the percentage of PDO wine to have improved performance relative to the other group (negative relation).

In panel A of table 1.9, I present the direct effect of the EBA Capital Exercise on performance. The outcome variables are operating profit margin and return on assets. According to the prediction, companies affected by the EBA Capital Exercise present lower performance relative to unaffected companies. The coefficients are not statistically significantly different from zero though. Next, I present the effect on performance mediated by the adjustment in product mix. Specifically, I analyze the effect that the adjustment in PDO wine production has on performance outcomes among the group of affected companies. It is important to recognize that this exercise is not clearly identified as deciding on how much PDO wine to produce is an endogenous decision. Even though, it may give some indication about the direction of the effect. In panel B of table 1.9, I show the results on this mediated effect. I interact the Post indicator with the percentage of PDO wine. Across the different specifications and outcome variables, I find a negative coefficient and significantly different from zero at least at 10% confidence level. Among the companies that were effected by the shock, those that adjusted the production of PDO wine downwards present higher performance ratios. This interpretation is consistent with the initial prediction and suggests that adjustment in production was an optimal response to the shock in the short-term.

Table 1.9: Effect on Performance

This table presents the impact on performance. In panel A, I estimate the direct impact of EBA Capital Exercise on performance. In panel B, I present the impact on performance mediated by the adjustment in the percentage of PDO wine. Analysis in panel B is restricted to treatment companies (companies with at least 50% of credit from EBA banks). From columns 1 to 3, the outcome variable is the operating profit margin scaled by total assets, where operating profit margin is defined as sales minus cost of goods sold. In columns 4 to 6, the outcome variable is return on assets defined as net income over total assets. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A: Direct Impact (Triple Difference)

	Operating Profit Margin			**Return on Assets**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.077	-0.041	-0.012	-0.083*	-0.051	-0.025
	[0.050]	[0.039]	[0.032]	[0.050]	[0.042]	[0.038]
Post × EBA Share	-0.002	-0.002	-0.008	-0.002	-0.001	-0.006
	[0.009]	[0.010]	[0.010]	[0.008]	[0.009]	[0.009]
EBA Share × Bank Dep.	-0.019			-0.009		
	[0.020]			[0.020]		
Post × Bank Dep.	0.032**	0.022	0.018	0.035**	0.024*	0.021
	[0.014]	[0.015]	[0.015]	[0.014]	[0.015]	[0.016]
Post	-0.004	-0.006	-0.011	-0.008	-0.010	-0.012
	[0.006]	[0.007]	[0.020]	[0.005]	[0.006]	[0.020]
EBA Share	-0.004			-0.004		
	[0.007]			[0.006]		
Bank Dep.	-0.026**			-0.035***		
	[0.011]			[0.011]		
Size $_{t-1}$		0.005	-0.004		0.013	0.007
		[0.019]	[0.021]		[0.021]	[0.026]
Internal Market $_{t-1}$		-0.014	-0.014		-0.012	-0.009
		[0.011]	[0.011]		[0.010]	[0.010]
Any PDO $_{t-1}$		0.018**	0.014*		0.018**	0.012*
		[0.008]	[0.008]		[0.008]	[0.007]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	3022	2096	1865	3022	2096	1865
No. Firms		405	382		405	382
Adjusted R^2	0.027	0.321	0.464	0.031	0.284	0.392

Panel B: Mediated Impact (conditioned on treatment companies)

	Operating Profit Margin			**Return on Assets**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × PDO (%)	-0.056**	-0.042**	-0.053*	-0.053*	-0.048*	-0.057*
	[0.027]	[0.021]	[0.029]	[0.027]	[0.026]	[0.030]
Post	0.018**	0.017***	-0.023	0.012	0.014**	-0.022
	[0.008]	[0.006]	[0.020]	[0.007]	[0.006]	[0.021]
PDO (%)	0.000	-0.030	-0.030	0.000	-0.034	-0.037
	[0.009]	[0.031]	[0.032]	[0.008]	[0.031]	[0.032]
Size $_{t-1}$		0.019	0.032		0.030	0.044
		[0.036]	[0.045]		[0.043]	[0.056]
Internal Market $_{t-1}$		-0.003	-0.007		0.008	0.008
		[0.015]	[0.017]		[0.014]	[0.017]
Any PDO $_{t-1}$		0.009	-0.003		0.015	-0.004
		[0.014]	[0.009]		[0.017]	[0.008]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	1285	1036	946	1285	1036	946
No. Firms		203	194		203	194
Adjusted R^2	0.01	0.542	0.523	0.012	0.411	0.39

1.5 Robustness tests

In this section I present robustness tests. A substantial part of the previous analysis relies on the existence of minimum ageing regulations as a proxy for production cycle length. In order to tackle the concern that these regions could differ in other potential dimensions, I looked at the within-company decision of producing red and white wines and show that companies adjust the production of the most restricted product (PDO red wine). As a robustness test, I try to proxy the duration of the production cycle with the most basic measure of inventory timing in the literature: Days Sales of Inventory ratio. If time is indeed a constraining factor, this variable should point towards a similar effect.[36] In table 1.10, I run the main specification among the group of companies with high and low Days Sales of inventory. The results are consistent with previous evidence. The adjustment occurs for companies with long inventories, i.e. those for which it takes longer to convert inventory into cash flow.

A second question is whether companies reduce PDO production due to higher certification costs. In table 1.11, I show that the reduction in PDO wine is accompanied by an increase in the production of PGI wine. If certification costs were a driving mechanism of the effect reported in the previous sections, I should expect companies to reduce PDO in favor of non-classified wine (the type that does not undergo certification processes). Therefore, I find evidence against this alternative mechanism.

I now perform several additional tests. In figure A5, I plot the histogram of average PDO wine production (in percentage) per company. There is clustering around 0 and 1. As a robustness test, I estimate the main model following a Tobit specification

[36]This variable needs to be interpreted carefully due to the possibility of measurement error. Many wineries have a vertically integrated productive process and part of the grapes used in wine production may come from own grape production. Given that this measure uses COGS, it is ultimately dependent on how companies report internal production. See Viana and Rodrigues (2006) for a survey of other accounting problems in the wine industry.

Table 1.10: Effect of Credit Constraints on Product Mix Decisions by Days Sales of Inventory Ratio

This table presents the impact of the EBA Capital Exercise on product mix decision by companies with high and low days sales of inventory. A company is defined as having high days sales of inventory if days sales of inventory ratio is above the median. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Days Sales of Inventory			
	High		**Low**	
	(1)	(2)	(3)	(4)
Post × EBA Share × Bank Dep.	-0.168*	-0.175**	-0.150	-0.080
	[0.096]	[0.072]	[0.114]	[0.083]
Post × EBA Share	0.063	0.085*	0.039	0.48
	[0.063]	[0.050]	[9.064]	[0.034]
EBA Share × Bank Dep.	-0.010		-0.085	
	[0.121]		[0.156]	
Post × Bank Dep.	0.001	0.101*	0.057	0.021
	[0.067]	[0.057]	[0.070]	[0.053]
Post	0.038	0.078	-0.023	-0.195
	[0.044]	[0.077]	[0.045]	[0.127]
EBA Share	0.017		0.030	
	[0.084]		[0.102]	
Bank Dep.	-0.000		0.134	
	[0.074]		[0.091]	
Size $_{t-1}$		0.002		0.066**
		[0.035]		[0.030]
Internal Market $_{t-1}$		0.040		0.077
		[0.082]		[0.061]
Any PDO $_{t-1}$		0.133**		0.067
		[0.060]		[0.047]
Firm FE	No	Yes	No	Yes
Year X Region	No	Yes	No	Yes
No. Observations	1248	974	1181	854
No. Firms		242		233
Adjusted R^2	0.001	0.762	0.010	0.859

with left- and right-censoring (table 1.12). Results are similar to the ones found in table 1.3. Additionally, I present estimates of a Probit model where the outcome variable equals one when the company produces 100% PDO wine. The likelihood of a treatment company producing 100% PDO wine reduced by 31 percentage points on average following the shock.

In table 1.13, I present results using alternative definitions of treatment. Specifically, I consider as treated all companies with at least one bank relationship with affected

Table 1.11: Can certification costs explain the adjustment in product mix?

This table presents the impact of the EBA Capital Exercise on different product categories. The outcome variable is the percentage of PDO wine (columns 1 and 2), percentage of PGI wine (columns 3 and 4) and percentage of non-classified wine (columns 5 and 6) in total production in year t. The first two columns correspond to columns 1 and 6 of table 1.3 and are presented here to facilitate comparison. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	% PDO		**% PGI**		**% Non-Classified**	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.136**	-0.151***	0.102*	0.118**	0.033	0.033
	[0.066]	[0.052]	[0.058]	[0.057]	[0.054]	[0.040]
Post × EBA Share	0.035	0.052*	-0.054*	-0.049	0.019	-0.003
	[0.037]	[0.030]	[0.032]	[0.034]	[0.032]	[0.030]
EBA Share × Bank Dep.	-0.028		-0.029		0.057	
	[0.101]		[0.088]		[0.067]	
Post × Bank Dep.	0.028	0.064*	-0.022	-0.026	-0.005	-0.038
	[0.043]	[0.038]	[0.040]	[0.043]	[0.033]	[0.029]
Post	0.012	0.039	0.001	-0.050	-0.013	0.011
	[0.026]	[0.070]	[0.022]	[0.074]	[0.023]	[0.021]
EBA Share	0.020		0.035		-0.055	
	[0.068]		[0.056]		[0.053]	
Bank Dep.	0.057		0.008		-0.065	
	[0.061]		[0.050]		[0.043]	
Size $_{t-1}$		-0.008		-0.003		0.011
		[0.023]		[0.024]		[0.018]
Internal Market $_{t-1}$		0.033		-0.034		0.000
		[0.046]		[0.044]		[0.032]
Any PDO $_{t-1}$		0.106***		-0.105***		-0.000
		[0.036]		[0.035]		[0.033]
Firm FE	No	Yes	No	Yes	No	Yes
Year X Region	No	Yes	No	Yes	No	Yes
No. Observations	2471	1855	2471	1855	2471	1855
No. Firms		381		381		381
Adjusted R^2	0.002	0.797	-0.001	0.767	0.004	0.747

banks (panel A), companies with at least 50% share of credit from affected banks (panel B) and a continuous variable of share of credit from affected banks weighted by each banks' distance to EBA Capital Exercise target capital ratio (Panel C). All alternative specifications document an overall negative impact of the shock on production of PDO wine and a stronger impact among regions with minimum ageing restrictions (column 2). Finally, I consider different time periods or samples. In panel A of table 1.14, I include 2014 in the analysis. In panel B, I exclude 2011 from the analysis. In panel C, I include companies that always produced PDO wine (these companies were initially removed

Table 1.12: Robustness test: Tobit and Probit

This table presents the impact of the EBA Capital Exercise on product mix decision. Here, I present the estimates based on tobit and probit specifications. In the first three columns (tobit), the outcome variable is the percentage of PDO wine in total production in year t. In the last three columns (probit), the outcome variable is a dummy variable equal to one if a company produces only PDO wine in year t. The later present marginal effect coefficients at the mean. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Tobit			**Probit (100% PDO)**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.223***	-0.208***	-0.246***	-0.217**	-0.224**	-0.312***
	[0.072]	[0.074]	[0.077]	[0.093]	[0.091]	[0.118]
Post × EBA Share	0.052	0.049	0.079	0.078	0.079	0.145*
	[0.046]	[0.048]	[0.050]	[0.054]	[0.054]	[0.082]
EBA Share × Bank Dep.	-0.004	0.079	0.039	0.139	0.143	0.130
	[0.124]	[0.120]	[0.086]	[0.089]	[0.090]	[0.096]
Post × Bank Dep.	0.057	0.068	0.089*	0.068	0.088	0.139*
	[0.045]	[0.046]	[0.048]	[0.058]	[0.056]	[0.074]
Post	-0.016	-0.021	0.117	-0.068*	-0.070*	0.081
	[0.030]	[0.031]	[0.189]	[0.036]	[0.039]	[0.228]
EBA Share	-0.001	-0.004	0.001	-0.062	-0.025	-0.031
	[0.083]	[0.080]	[0.057]	[0.062]	[0.061]	[0.072]
Bank Dep.	0.027	-0.017	-0.021	-0.018	-0.045	-0.062
	[0.078]	[0.076]	[0.055]	[0.055]	[0.058]	[0.064]
Size $_{t-1}$		-0.038**	0.000		-0.046***	-0.028**
		[0.016]	[0.012]		[0.013]	[0.014]
Internal Market $_{t-1}$		-0.001	-0.041		-0.018	-0.065
		[0.063]	[0.060]		[0.057]	[0.072]
Any PDO $_{t-1}$		0.304***	0.324***		0.140***	0.115**
		[0.032]	[0.033]		[0.031]	[0.045]
Firm FE	No	No	No	No	No	No
Year X Region	No	No	Yes	No	No	Yes
No. Observations	2471	1991	1855	2471	1991	1670
No. Firms	422	392	381			

from the sample). The results are robust to any of these alternative specifications.

Table 1.13: Robustness: Alternative Definitions of Treatment

This table presents the impact of the EBA Capital Exercise on product mix decision using alternative definitions of treatment. In panel A, I define as treatment group all companies with at least one bank relation with affected banks. In panel B, treatment is a dummy variable equal to one whenever a company has a share of credit from affected banks higher than 50%. Panel C uses a continuous measure of exposure to treatment using each bank's distance to EBA target capital ratio. Specifically, I take the logarithm of the weighted average share of credit from affected banks whose weights correspond to each creditor's distance the EBA threshold. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). All regression include the following controls: Size$_{t-1}$, Internal Market $_{t-1}$ and Any PDO $_{t-1}$. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A: At least 1 bank relationship with EBA banks [binary]

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × Treat. × Bank Dep.	-0.101*	-0.229**	-0.048
	[0.059]	[0.095]	[0.070]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1855	712	1143
No. Firms	381	168	213
Adjusted R^2	0.796	0.8	0.782

Panel B: At least 50% share of credit from affected banks [binary]

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × Treat. × Bank Dep.	-0.126***	-0.204***	-0.093
	[0.045]	[0.068]	[0.056]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1855	712	1143
No. Firms	381	168	213
Adjusted R^2	0.797	0.801	0.782

Panel C: Log(Average share of credit from affected banks weighted by bank's distance to EBA threshold) [continuous]

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × Treat. × Bank Dep.	-0.036*	-0.048*	-0.033
	[0.019]	[0.025]	[0.024]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1398	540	858
No. Firms	381	129	158
Adjusted R^2	0.811	0.839	0.788

Table 1.14: Robustness: Different time period or sample

This table presents robustness tests using different time periods or samples. In panel A, I include 2014. In panel B, I exclude 2011 from the analysis (EBA Capital Exercise announcement). In panel C, I present the results when including companies that have produced always 100% PDO wine throughout the period. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). All regression include the following controls: Size_{t-1}, Internal Market $_{t-1}$ and Any PDO $_{t-1}$. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A: Including 2014

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × EBA Share × Bank Dep.	-0.156***	-0.262***	-0.102
	[0.049]	[0.074]	[0.064]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	2182	861	1321
No. Firms	410	192	218
Adjusted R^2	0.795	0.815	0.773

Panel B: Excluding 2011

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × EBA Share × Bank Dep.	-0.169***	-0.293***	-0.110
	[0.061]	[0.093]	[0.079]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1571	610	961
No. Firms	379	167	212
Adjusted R^2	0.799	0.806	0.784

Panel C: Including companies that produce 100% PDO throughout the period (initially removed)

	All Sample	Min. Ageing Restriction	No Min. Ageing Restriction
	(1)	(2)	(3)
Post × EBA Share × Bank Dep.	-0.136***	-0.216***	-0.103
	[0.050]	[0.079]	[0.064]
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year X Region	Yes	Yes	Yes
No. Observations	1942	737	1205
No. Firms	403	178	225
Adjusted R^2	0.806	0.81	0.791

1.6 Conclusion

Financial frictions may impact project maturity decisions of companies. Inefficiencies may arise as companies favor short-term projects which provide short-term cash-flow at the expense of long run cash flows.

I find that affected companies adjust product mix as a response to the credit contraction induced by the EBA Capital Exercise. Specifically, affected companies reduce the percentage of PDO wine on total company's production. Next, I analyze the role of the production cycle mechanism. In other words, I investigate whether the reduction in the production of PDO wine is driven by the need of shorter production cycles and eventually quicker cash conversion. To do so, I split companies according to regions where there is a minimum ageing regulation on the production of PDO wine.These regulations establish a lower bound on the production cycle length. In regions where a regulation is in place, companies need to shift production of the constrained product to other products in order to produce faster. I show that the impact found in the full sample is entirely driven by companies in these regions. The contrasting results among the two types of regions suggest that the ageing restriction plays a role and constrains companies in their product market decisions.

I investigate why adjustment in product mix is a natural response to an adverse financial shock. Alternatively, a more direct way of alleviating financial constraints and generating cash-flow in the short-term would be increasing sales (selling current inventory). Hence, I expect the adjustment in production to be stronger among companies with lower inventories. I find evidence that this is the case by splitting companies with high and low inventory levels. These results point towards a complementarity between production decisions and levels of current stock.

Finally, I analyze whether affected companies that have adjusted their product mix

perform better than affected companies whose production remained unaltered. I find evidence that companies that adjusted production of PDO wine downwards present higher performance ratio. Overall, this result suggests that the adjustment in production was an efficient response to the shock on the companies' perspective.

My results suggest that the adverse impact of financial constraints on product markets may increase with longer, less flexible, production cycles.

Appendix of Chapter 1

Figure A1: Wine regions in Portugal

Geographical distribution of wine regions in Portugal. *Source*: Wines of Portugal: http://www.winesofportugal.info/pagina.php?codNode=18012&market=1

Figure A2: Example of seal of guarantee - PDO and IGP Alentejo

This figure illustrates the seal of guarantee issued by Alentejo regional industry regulator (CVRA). On the left (right), it is presented the PDO (PGI) seal of guarantee. *Source:* Wines of Alentejo: https://www.vinhosdoalentejo.pt/en/cvr-alentejana/certification-process/

Selos de Garantia

Denominação de Origem Controlada Alentejo.

Vinho Regional Alentejano.

Figure A3: Representation of wine-making process

This diagram depicts the main decision nodes in the wine-making process. Solid arrows indicate the main production flows. Dashed arrows indicate other, less relevant, in or outflows. This figure was constructed by the author based on conversations held with winemakers.

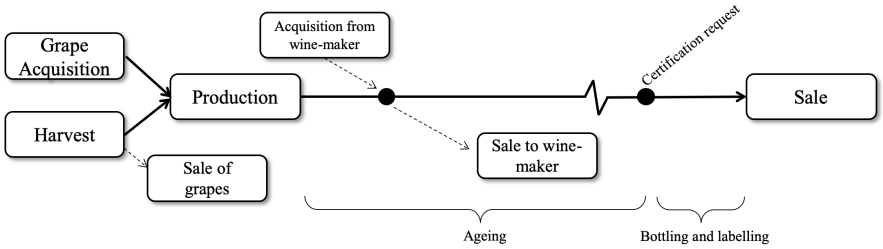


Figure A4: Evolution of PDO wine production in percentage of total

This figure displays the evolution of median (left) and mean (right) PDO wine production in percentage of total annual production. In the top figures, treatment is defined as companies with a share of credit from affected banks above 50%. In the bottom figures, treatment is defined as the intersection between companies with a high share of credit from affected banks (above 50%) and bank dependent companies. Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Vertical lines delimit the period between the EBA Capital Exercise announcement and deadline.

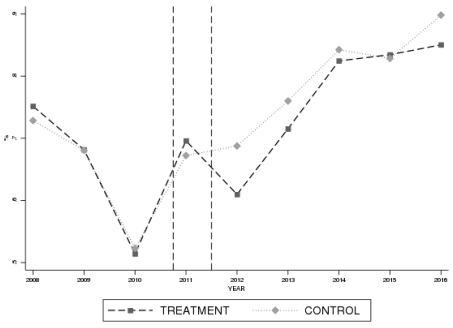
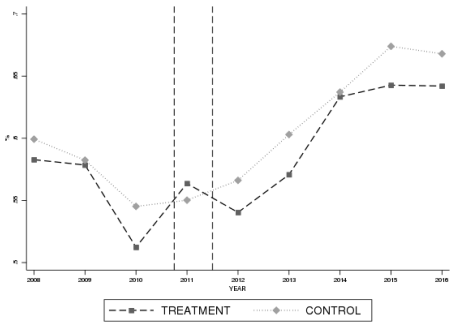
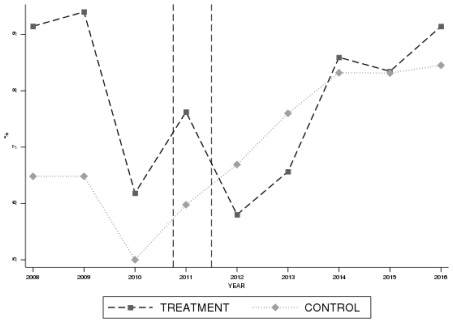
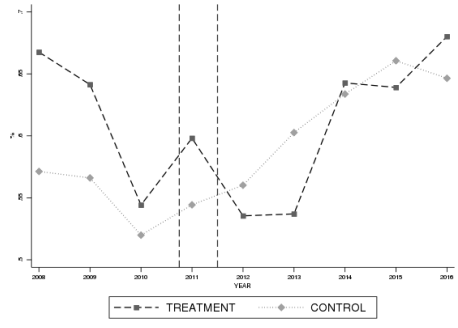
Treatment	**Median**	**Mean**
High EBA Share		
High EBA Share + Bank Dependent		

Figure A5: Histogram of PDO production

This figure depicts an histogram of the PDO wine production (in percentage of total production). It is computed based on each company's mean over time.

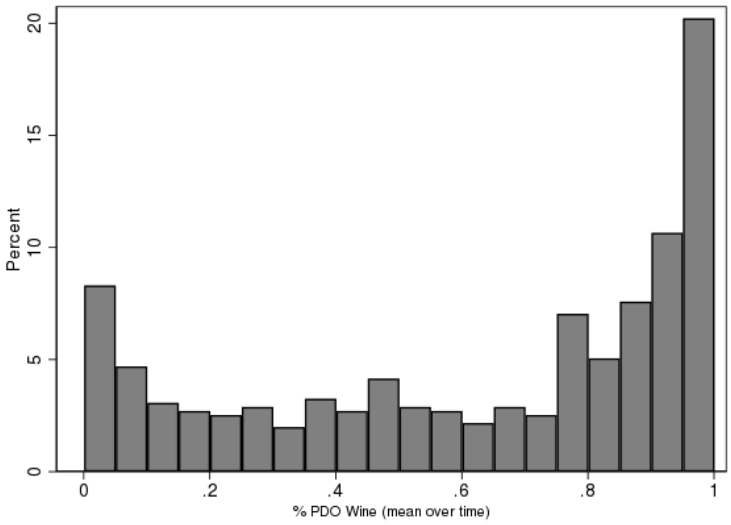


Table A1: Description of Production Stages

This table describes each of the main stages in the wine production process (see figure A3).

Stage	Description
Harvest	Harvesting is the first step in the wine making process. Grapes should be harvested at the precise time, preferably when physiologically ripe for the intended type of wine. In Portugal, it occurs between August and October. Wineries can produce grapes in their own estates or acquire grapes from external winegrowers or cooperative. Traditionally, vineyard management and wine-making were separated activities. Nowadays, winemakers own large vineyard plots. Yet, acquiring grapes from winegrowers is still very common as it allows wineries, in particular large ones, to acquire raw-materials necessary to satisfy large production levels. This is the case in Portugal, particularly due to tight permits for new vineyard planting and limits on maximum yield. The appellation systems (*appellation contrôlée*) in place in many European Countries intend to control production and ensure quality standards in each wine region. These regulations often start with vineyard planting and classification. Typically, planting new vineyards can only be carried out following regulators' permits and is subject to narrow annual quotas. Moreover, each vineyard can be classified as PDO or PGI. This classification depends on vineyard characteristics, number of vines and grape varieties. This classification is usually fixed throughout the vineyard useful life. In order to produce wine with any of these certifications, grapes must come from vineyards classified in the same way. This is a necessary but far from sufficient step (more details about certification are given below). Regional authorities keep track of grape classification and respective quantities declared by any agent that produces grapes for commercial purposes ('*current accounts*'), as well as sales between agents ('*movements*'). Every time a *movement* occurs, the *current accounts* of both seller and buyer are updated. At this stage, companies buy and sell grapes with different classifications to attain the desired product mix in each year.
Production	Production occurs right after the harvest and includes crushing and pressing, fermentation and clarification. By the end of this stage, winemakers are required to declare total production levels by type and color. They also report the certification a given quantity is suitable for (PDO, PGI or non-classified wine). At this point, this is based on the classification of grapes it was made from. The *current accounts* are updated with this new information and it limits the quantity a company can request for certification.
Ageing and Bottling	The last stage of the wine production process involves ageing and bottling. Depending on the type and color of wine, the winemaker can bottle wine immediately after clarification or can be given additional maturation (usually in oak barrels or stainless steel tanks). Ageing is considered to follow two phases. The first – maturation – refers to the period between fermentation and bottling. It frequently lasts from 6 to 24 months. The second phase – reductive ageing - starts with bottling and occurs in the absence of oxygen. In Portugal, wineries are required to declare inventory levels and respective characteristics (year of harvest, type, color, quantity) every year to regional regulators.
Certification	Companies can request certification and the respective numbered seals of guarantee as soon as the wine has been produced. The timing may depend on technical factors, such as the optimal ageing point or the existence of minimum ageing regulations, or economic variables, such as market dynamics (e.g. demand). The first step of a certification procedure is the validation of the request against company's *current accounts*. Then, a sample of wine is subject to physicochemical analysis at the regulators' laboratories. Simultaneously, a sensory analysis is performed by a tasting panel. After that, if all requirements are met, the wine is certified as PDO or PGI and the respective seals of guarantee are issued. The newly certified wine is ready to be introduced in the market. In opposition, producers may request declassification of wine previously registered in their current accounts as suitable for PDO or PGI wine. The request must be submitted to regional regulators who may approve or refuse it (IVV, 2018). Further details on the institutional organization of the wine sector can be found on Decree-law 212/2004, Ministry of Agriculture, Portugal https://dre.pt/web/guest/pesquisa/-/search/479875/details/maximized.

Table A2: Description of products per wine regions in Portugal

This table describes products in each wine region in Portugal with regard to certification and type. It also presents the trading name of each product. From column 4 to 8, it reports the mandatory minimum ageing period (in months) for each type of wine (and color in the case of still wines). 'x' indicates that PDO or PGI certification exists for each type of wine (still, liqueur or sparkling) in each region. I replace 'x' by the corresponding minimum ageing period (in months) for wines with mandatory minimum ageing. For the sake of simplicity I omit semi-sparkling type and rose color. Last column indicates the regions where there are minimum ageing restrictions on PDO production cycle. Minimum ageing periods for PDO Porto (Port wine) can vary from several months to decades, depending on the sub-type and quality. Due to the complexity of this product regulation, I prefer not to report any minimum ageing period [a]. *Source:* IVV technical specifications (http://www.ivv.gov.pt/np4/528/) and IVV yearbook 2016 (http://www.ivv.gov.pt/np4/Anuário).

Region	**Certif.**	**Trading name**	**Min. Ageing**				**Min. Ageing Restriction**
			Still		**Liq.**	**Spark.**	
			Red	**White**			
Vinho Verde	PGI	Minho	x	x	x	x	No
	PDO	Vinho Verde	x	x		9*	
Trás-os-Montes	PGI	Trasmontano	x	x			No
	PDO	Trás-os-Montes	x	x	x	x	
Douro	PGI	Duriense	6	x		9	Yes
	PDO	Douro	8	1	18	9	
	PDO	Porto			**[a]**		
Távora-Varosa	PGI	Terras de Cister	x	x		x	No
	PDO	Távora-Varosa	x	x		9	
Bairrada	PGI	Beira Atlântico	x	x		x	No
	PDO	Bairrada	x	x	x	9	
Beira Interior	PGI	Terras da Beira	x	x		x	No
	PDO	Beira Interior	x	x		x	
Dão	PGI	Terras do Dão	x	x		x	Yes
	PDO	Dão	8	x		9	
	PDO	Lafões	6	x			
Lisboa	PGI	Lisboa	x	x	x	x	Yes
	PDO	Encostas D'Aire	8	x			
	PDO	Óbidos	8	x		9	
	PDO	Alenquer	8	x			
	PDO	Arruda	14	3			
	PDO	Torres Vedras	8	3			
	PDO	Bucelas	x	x		x	
	PDO	Carcavelos			30		
	PDO	Colares	24	9			
Tejo	PGI	Tejo	x	x		x	Yes
	PDO	DoTejo	6	x	x	9	
Pen. de Setúbal	PGI	Pen. de Setúbal	x	x	x	x	No
	PDO	Setúbal			18		
	PDO	Palmela	x	x	x	x	
Alentejo	PGI	Alentejano	x	x	x	x	No
	PDO	Alentejo	x	x	x	x	
Algarve	PGI	Algarve	x	x	x	x	Yes
	PDO	Lagos	6	x			
	PDO	Portimão	6	x			
	PDO	Lagoa	6	x			
	PDO	Tavira	6	x			
Madeira	PGI	Terras Madeirenses	x	x			Yes
	PDO	Madeira			2		
	PDO	Madeirense	6	x			
Açores	PGI	Açores	8	6	8	x	Yes
	PDO	Biscoitos	6	6	24	x	
	PDO	Graciosa	6	6	24	x	
	PDO	Pico	6	6	24	x	

Table A3: EBA Capital Exercise Impact on Firms' Total Credit

This table presents the impact of the EBA Capital Exercise on firms' total credit. The outcome variable is non-current liabilities scaled by (lagged) total assets. Columns 1 and 2 present the results for the entire sample. In columns 3 and 4, I condition the analysis on companies that are bank dependent. Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). In columns 5 and 6, I present the results on a triple difference interaction with Post, EBA Share and Bank Dependent. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Size_{t-1} is logarithm of total assets in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	All Sample		**Bank Dependent**		**All Sample**	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share	-0.108**	-0.120**	-0.260**	-0.261**	0.003	-0.014
	[0.051]	[0.054]	[0.120]	[0.114]	[0.016]	[0.020]
Post	0.043***	0.075***	0.103***	0.159***	-0.006	0.022
	[0.016]	[0.027]	[0.033]	[0.050]	[0.011]	[0.019]
Post × EBA Share × Bank Dep.					-0.263**	-0.246**
					[0.121]	[0.110]
Post × Bank Dep.					0.109***	0.117***
					[0.034]	[0.038]
Size_{t-1}		-0.143*		-0.218**		-0.141*
		[0.075]		[0.107]		[0.073]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
No. Observations	2672	2672	1096	1096	2648	2648
No. Firms	436	436	187	187	428	428
Adjusted R^2	0.356	0.381	0.307	0.352	0.362	0.387

Table A4: Comparison of Means

Panel A of this table presents a comparison of means between the sub-sample of companies with at least 50% of credit from affected banks and the sub-sample of companies with less than 50% of credit from affected banks at the time of the EBA Capital Exercise announcement (October 2011). Companies with no credit outstanding are excluded from the analysis. Panel B presents a mean comparison between companies inside and outside regions where minimum ageing restrictions are in place. All means are reported at the end of 2011. In the last column I present the difference in means. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Panel A: At least 50% Share of Credit From Affected Banks

	At least 50% Share of Credit From Affected Banks				
	Yes		**No**		**Diff.**
	Obs.	**Mean**	**Obs.**	**Mean**	
Total Assets	201	5765681.3	197	3592126.5	-2173554.8***
Nr. Employees	201	26.98	197	14.77	-12.21*
Sales	201	2222195.8	197	1363153.4	-859042.41**
ROA	199	-0.02	195	-0.03	-0.01
Leverage	201	0.73	197	0.67	-0.06
Inv./Assets	201	0.26	197	0.25	-0.01
Days in inv.	199	1534.02	192	1361.45	-172.57
Bank Rel.	201	3.27	197	3.25	-0.02
Largest bank rel.	201	0.76	197	0.76	0.00
Pct. short-term	182	0.48	185	0.43	-0.05
Region Min. Ageing Restriction	193	0.50	196	0.44	-0.06
PDO	162	0.55	154	0.55	0.00
Harvest for own production	121	0.91	109	0.91	0.00
Red wine	162	0.59	154	0.51	-0.08*
Bottled wine	176	0.20	161	0.20	0.00
No. bank relationships:					
All	201	3.28	197	3.28	0.00
With affected banks	201	1.69	197	0.80	-0.89***
With affected banks:					
At least 1 relation	201	1.00	197	0.53	-0.47***
Share of credit	201	0.86	197	0.11	-0.75***

Panel B: Region with Minimum Ageing Restriction

	Region with Minimum Ageing Restriction				
	Yes		**No**		**Diff.**
	Obs.	**Mean**	**Obs.**	**Mean**	
Total Assets	207	4381979.2	259	3489156.2	-892823
Nr. Employees	207	18.42	259	14.16	-4.258
Sales	207	1546460	259	1293609.3	-252850.7
ROA	203	-0.02	254	-0.04	-0.028
Leverage	207	0.75	259	0.70	-0.052
Inv./Assets	207	0.25	259	0.23	-0.012
Days in inv.	202	1779.21	252	1294.15	-485.1*
Bank Rel.	196	3.05	241	2.74	-0.308
Largest bank rel.	185	0.77	211	0.76	-0.010
Pct. short-term	172	0.50	191	0.43	-0.069
Region Min. Ageing Restriction				-	
PDO	126	0.43	240	0.63	0.198***
Harvest for own production	96	0.88	185	0.86	-0.028
Red wine	126	0.72	240	0.47	-0.253***
Bottled wine	166	0.17	206	0.24	0.066**
No. bank relationships:					
All	183	3.32	206	3.19	-0.128
With affected banks	183	1.21	206	1.24	0.025
With affected banks:					
At least 1 relation	183	0.77	206	0.76	-0.008
Share of credit	183	0.51	206	0.45	-0.055

Table A5: Effect of Credit Constraints on Product Mix Decisions (double difference)

This table presents the impact of the EBA Capital Exercise on product mix decision using a double-difference specification. The analysis is conditioned on bank dependent companies. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share	-0.100*	-0.098**	-0.099**	-0.081	-0.089**	-0.085**
	[0.054]	[0.047]	[0.045]	[0.051]	[0.045]	[0.043]
Post	0.040	0.039	-0.028	0.050	0.041	0.126
	[0.035]	[0.033]	[0.147]	[0.035]	[0.033]	[0.100]
EBA Share	-0.008			0.041		
	[0.074]			[0.063]		
Size $_{t-1}$				-0.014	-0.016	0.004
				[0.017]	[0.037]	[0.037]
Internal Market $_{t-1}$				-0.063	0.003	-0.008
				[0.100]	[0.070]	[0.086]
Any PDO $_{t-1}$				0.526***	0.108*	0.151***
				[0.040]	[0.055]	[0.056]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	1001	1001	978	803	803	782
No. Firms		182	181		166	165
Adjusted R^2	0.001	0.686	0.696	0.234	0.713	0.73

Table A6: Effect of Credit Constraints on Product Mix Decisions by Regions with Minimum Ageing Restriction (double difference)

This table presents the impact of the EBA Capital Exercise on product mix decision of companies inside or outside regions with minimum ageing restriction using a double-difference specification. The analysis is conditioned on bank dependent companies. The outcome variable is the percentage of PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size_{t-1} is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Regions with Minimum Ageing Restriction					
	Yes			**No**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share	-0.213***	-0.187***	-0.165**	-0.032	-0.038	-0.043
	[0.080]	[0.068]	[0.072]	[0.073]	[0.057]	[0.054]
Post	0.121**	0.100*	0.067	-0.007	0.013	0.114
	[0.051]	[0.051]	[0.051]	[0.045]	[0.041]	[0.099]
EBA Share	0.081			-0.059		
	[0.106]			[0.102]		
Size $_{t-1}$		-0.080	-0.034		0.004	0.016
		[0.080]	[0.088]		[0.039]	[0.044]
Internal Market $_{t-1}$		0.039	-0.047		-0.006	0.043
		[0.113]	[0.157]		[0.087]	[0.101]
Any PDO $_{t-1}$		0.047	0.057		0.146**	0.208***
		[0.099]	[0.114]		[0.065]	[0.060]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	400	303	289	597	496	493
No. Firms		69	69		96	96
Adjusted R^2	0.004	0.729	0.735	0	0.705	0.731

Table A7: Effect of Credit Constraints on Product Mix Decisions by Wine Color (double difference)

This table presents the impact of the EBA Capital Exercise on product mix decision by wine color using a double-difference specification. The analysis is conditioned on bank dependent companies. The analysis is conditioned on companies operating in regions where a minimum ageing restriction is in place. The outcome variable is the percentage of red (first three columns) or white (last three columns) PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size$_{t-1}$ is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Color					
	Red			**White**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share	-0.174**	-0.125	-0.158**	-0.039	-0.062	-0.007
	[0.081]	[0.081]	[0.063]	[0.037]	[0.048]	[0.024]
Post	0.100**	0.076	0.053	0.020	0.024	0.014
	[0.048]	[0.046]	[0.047]	[0.020]	[0.022]	[0.011]
EBA Share	0.073			0.008		
	[0.092]			[0.059]		
Size $_{t-1}$		-0.068	-0.007		-0.012	-0.027
		[0.074]	[0.086]		[0.026]	[0.030]
Internal Market $_{t-1}$		0.143	0.004		-0.104	-0.051
		[0.123]	[0.120]		[0.099]	[0.062]
Any PDO $_{t-1}$		0.057	0.075		-0.010	-0.017
		[0.087]	[0.098]		[0.021]	[0.025]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	400	303	289	400	303	289
No. Firms		69	69		69	69
Adjusted R^2	0.003	0.698	0.746	-0.006	0.672	0.805

Table A8: Effect of Credit Constraints on Product Mix Decisions in Regions with No Minimum Ageing Restriction

This table presents the impact of the EBA Capital Exercise on product mix decision by wine color. The analysis is conditioned on companies operating in regions where there is no minimum ageing restriction in place. The outcome variable is the percentage of red (first three columns) or white (last three columns) PDO wine in total production in year t. Post is a binary variable equal to one from 2012 onward. EBA Share is the share of credit from affected banks at the time the announcement (October 2011). Bank dependent is a dummy variable equal to 1 when the company has a high debt ratio (non-current liabilities scaled by total assets above the median). Size$_{t-1}$ is logarithm of total assets in the previous year. Internal Market $_{t-1}$ is the percentage of sales in the domestic market. Any PDO $_{t-1}$ is a dummy variable equal to 1 when the company has produced some PDO wine in the previous year. Additional time invariant variables and interactions are captured by fixed effects and are therefore omitted. The sample covers the period 2006-2013. Robust standard errors clustered at firm level are shown in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

	Color					
	Red			**White**		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × EBA Share × Bank Dep.	-0.107*	-0.065	-0.068	0.016	-0.033	-0.044
	[0.057]	[0.042]	[0.043]	[0.076]	[0.056]	[0.055]
Post × EBA Share	0.018	0.029	0.029	0.041	0.030	0.029
	[0.038]	[0.031]	[0.032]	[0.035]	[0.022]	[0.024]
EBA Share × Bank Dep.	-0.014			0.015		
	[0.084]			[0.139]		
Post × Bank Dep.	0.009	0.015	0.019	-0.017	0.010	0.010
	[0.035]	[0.028]	[0.027]	[0.048]	[0.040]	[0.038]
Post	0.019	0.003	0.045	-0.019	-0.013	-0.005
	[0.023]	[0.019]	[0.064]	[0.026]	[0.014]	[0.020]
EBA Share	0.023			-0.083		
	[0.060]			[0.090]		
Bank Dep.	-0.034			0.013		
	[0.045]			[0.079]		
Size $_{t-1}$		-0.019	-0.013		0.006	0.014
		[0.021]	[0.022]		[0.016]	[0.018]
Internal Market $_{t-1}$		-0.018	-0.011		-0.024	-0.005
		[0.045]	[0.046]		[0.041]	[0.043]
Any PDO $_{t-1}$		0.111***	0.114***		0.035	0.034
		[0.036]	[0.039]		[0.026]	[0.027]
Firm FE	No	Yes	Yes	No	Yes	Yes
Year X Region	No	No	Yes	No	No	Yes
No. Observations	1404	1181	1143	1404	1181	1143
No. Firms		215	213		215	213
Adjusted R^2	0.009	0.736	0.739	0.001	0.89	0.894

Chapter 2

The Impact of Financial Education of Managers on Medium and Large Enterprises — A Randomized Controlled Trial in Mozambique

This chapter was written in co-authorship with Cláudia Custódio and Daniel Metzger. We thank Miguel Ferreira, Raj Iyer, Antoinette Schoar, John van Reenen, Pedro Vicente as well as seminar participants at Arizona State University, Erasmus University Rotterdam, Imperial College London, Nova SBE Lisbon, LBS summer symposium, University of Geneva SFI, Toulouse School of Economics, and University of Cologne for very helpful comments. We thank Adriana Ravara, Mattia Fracchia, Hetal Kanji, and Katherine Velasquez Rodriguez for outstanding research assistance. We thank the IGC team in Mozambique (Jorrit Oppewal and Novella Maugeri), NOVAFRICA (Raquel Fernandes), and Imperial College London for their administrative support. We also thank IGC for financial support.

2.1 Introduction

Management practices help to explain differences in firm productivity and profitability, and development levels across countries (e.g., Bloom and Van Reenen (2011); Bloom et al. (2013)). This literature mostly focusses on lower or middle management of larger corporations or on founders/CEOs of small or micro-enterprises. There is no quasi-experimental evidence on executives of larger companies even though their potential impact on economic development is also larger as they effectively control a large part of the economy. In this paper, we conduct a randomized controlled trial (RCT) with top-level executives of medium and large companies in Mozambique, who participate in an executive education programme in finance. The programme focus on investment and capital allocation decisions as well as firm financial policies. While financial decisions are irrelevant in a frictionless world, the ability to make optimal financial decisions may have a positive impact on firm value in contexts where financial frictions are potentially severe such as in developing economies. The World Bank Enterprise Survey (2018) identifies "Access to Finance" together with "Corruption" as the biggest obstacles for firms in Mozambique, followed by "Practices of the informal sector", "Crime", and "Political Instability". In terms of financing, only 10% of the firms have a bank loan or a line of credit, compared to about 44% that refer to still need a bank loan. More than 21% of the firms had a recent loan application that was rejected. One reason could be collateral requirements as more than 90% of the loans required a collateral, with an average of 271% of the loan value being requested as collateral.

We document a positive treatment effect of the executive education programme on financial policies and firm profitability. Using survey data and financial accounting data from one of the world's largest accounting firms (KPMG), we find that managers adjust some firm financial policies. The largest treatment effects are for short term

financial policies related to working capital, which generates a positive impact on cash flows due to reduction in accounts receivable and inventories but no observed change in accounts payable. We also find a smaller yet significant positive change on investment in fixed capital, in response to the treatment. We find these policy changes to improve firm performance, which is consistent with efficiency gains.

Our findings suggest that individual CEOs, and, in particular, their financial education matter for corporate policies and, ultimately, for corporate performance. These findings are consistent with Bertrand and Schoar (2003) who argue that individual CEOs help to explain observed heterogeneity in management practices and corporate policies, and conclude that CEOs possess different "styles".[37] With respect to financial expertise, existing research shows that managers' financial expertise impacts revenues and survival rates of corporations in the context of small and micro-entrepreneurs in developing countries (Drexler et al., 2014; Anderson-Macdonald et al., 2014) and is correlated with firm financial policies in developed countries such as the U.S. (Custodio and Metzger, 2014). Custodio and Metzger (2014) document that firms run by financial expert CEOs hold less cash, more debt, and engage more intensively in share repurchases, especially when there is a relative tax advantage with respect to dividends payments. They also find that financial expert CEOs are more aware of common capital budgeting decisions mistakes. Overall, the evidence suggests that the impact of financial expertise of CEOs on economic outcomes is potentially large. Our study makes three contributions to the literature. First, we show in a RCT setting that individual CEOs, and in particular their financial skills, have real effects on firm financial policies and profitability. Second, we show that Executive Education matters, and that relatively low-cost interventions,

[37]There is substantial research linking CEO styles to preferences and traits (e.g., Malmendier and Tate (2005, 2008), Malmendier et al. (2011), Kaplan et al. (2012), Graham et al. (2013), Hirshleifer et al. (2012)), to education (e.g., Bertrand and Schoar (2003), and Malmendier and Tate (2005)), or to industry-specific work experience (e.g., Custodio and Metzger (2013, 2014) and Custódio et al. (2013); Custodio et al. (2017).

such as an 18-hour MBA-style finance executive education course, help to build-up finance expertise. Last, our results suggest that improving short term financial policies such as working capital can potentially relax financial constraints, at least in the short run, by improving firm liquidity.

We start by documenting substantial heterogeneity in financial expertise by CEOs in Mozambique. About 43% of the CEOs have a background in finance, either by education or work experience. When analyzing financial practices in firms with and without financial expert CEOs, we find large differences in their practices. For example, we find significant differences between these two groups in the way they evaluate investment projects. While a large majority of CEOs with a background in finance is making use of sophisticated valuation techniques such as net present value (NPV) (70%), or conducts sensitivity analysis (63%), this is relatively uncommon for CEOs without such a background. Only 25% of CEOs with no financial background use NPV, and only 33% of them perform sensitivity analyses in their capital budgeting calculations. At the same time, they are more likely to use less sophisticated valuation techniques such as hurdle rates (63%). These findings are consistent with US evidence by Bertrand and Schoar (2003) and Custodio and Metzger (2014) who found CEOs with MBAs or financial expertise are much more likely to follow financial theory and textbook rules, and to avoid common mistakes such as using a unique firm cost of capital irrespective of the nature of the project (the WACC fallacy).[38] While these results are suggestive of an effect of financial expertise on financial policies, omitted variables may bias the coefficients.

In order to identify a potential treatment effect of financial expertise on firm policies, one would need to observe exogenous variation of financial expertise across firms. One way

[38]The use of companywide discount rates to evaluate investment projects rather than project-specific ones has been called the weighted average cost of capital (WACC) fallacy (Graham and Harvey, 2001; Krüger et al., 2015).

of doing so would be an actual random allocation of CEOs to firms. Unfortunately, this type of experiment is not feasible in the context of large firms. We propose a different solution, by randomizing financial education of top managers, and, at the same time, keeping the match between CEOs and firms constant. We "treat" managers with financial expertise by offering a free MBA-style course on corporate finance to top managers of 93 medium and large firms in Mozambique. Similar approaches are commonly employed in the development economics literature and have been used to measure the impact of financial literacy on revenue and survival rates for small and micro-entrepreneurs (Drexler et al., 2014; Anderson-Macdonald et al., 2014), but have not been applied to larger companies. An exception is Bloom et al. (2013) who use a randomized controlled experiment to measure the effects of management practices on the productivity of large plants in India.[39] However, they focus on lower-tier, plant managers rather than on executives, and they do not study financial education and financial policies. Our paper is the first RCT project in economics whose intervention targets executives of relatively large companies.[40]

To address concerns of endogenous selection into the treatment, we randomly staggered the timing of the treatment of firms that expressed their interest in participating in the executive education programme. Firms were randomly allocated into two cohorts: a treatment group and a control group. Randomization was done in a stratified way so that both groups are balanced in terms of industries. The first cohort - the treatment group - received the treatment in May 2017, while the second cohort - the control group - received the same treatment in November 2018 / April 2019. We offered the

[39]Other experiments find mixed evidence of the impact of basic business training to micro and small enterprises in developing countries (Karlan and Valdivia, 2011; Bruhn and Zia, 2013; Bruhn et al., 2012; Karlan et al., 2012).

[40]Their experiment was carried out in 17 firms operating 28 plants; this relatively low number of observations reflects the difficulty to obtain big samples in the context of RCTs with large corporations. In this respect, a sample size of 93 firms appears notable.

control group the course to provide an incentive to participate in the experiment, and to make detailed financial data available. During the fifteen-month period, both firms were contacted to collect financial data and to conduct follow-up surveys on financial practices. One survey was conducted immediately after the intervention to evaluate the intentions of executives to change their behavior, and a second survey was conducted 15 months after the intervention to evaluate implemented changes.

The main results can be summarized as follows: i) treated firms report high intentions to change financial policies after the participation in the course (73% of the firms intend to adjust their working capital management, 70% risk management, 42% valuation techniques and 48% capital structure). The survey also reveals that a sizeable fraction of firms is not able to adjust their capital structure (32.5%), risk management and valuation practices (17.5% each), mostly because they are subsidiaries and these policies are set somewhere else in the business group. ii) 30.8% of treated firms report that they have implemented changes in working capital management 15 months after the treatment. Corresponding figures for other financial policies are lower (11.5% for capital structure decisions and valuation, and 7.7% for risk management). Moreover, firms report that they implemented these changes because of the treatment (i.e., the course they participated in 15 months before). While these results are suggestive of a treatment effect, we also make use of the control group to address the concern that we may be capturing for instance a pure time-effect. Indeed, it might be the case that changes in the economy led companies to change their financial policies, irrespective of the treatment. When we compare mean differences between treatment and control groups (using a one-sided t-test), we find a large and significant difference for changes in working capital management (significant at the 1%-level) and changes in capital structure and risk management (significant at the 10%-level). iii) we make use of accounting data to validate the survey evidence and to analyze potential implications for firms' ef-

ficiency. Using a difference-in-difference estimator, we find evidence that is supportive of the survey results. We find a large and negative effect on working capital: working capital decreases by 0.86 standard deviations for the treated firms when compared to the control group. When decomposing the effect, we find that treated firms decrease their collection period, reducing account receivables, as well their inventories. These changes are expected to have a positive effect on liquidity in the short run. We do not find any effect on cash holdings or leverage. Consistent with these findings, we find that treated firms use that cash inflow, at least partially, to increase capital expenditures.

Whether these changes have led to policies that are more efficient or not is not clear ex-ante. For instance, by collecting receivables too quickly or reducing inventory too much, sales might be adversely affected. To test whether firms have indeed moved towards more efficient policies as a response of the treatment, we analyze whether treated firms show better performance relative to the control group. Given that most firms are private, we do not observe their market values.[41] Hence, we rely on accounting ratios to measure efficiency. Analyzing return on assets (ROA), we find that treated firms' ROA increases by 0.37 standard deviations compared to control firms. We also find that return on invested capital (ROIC) improves, whereas at the same time, we do not find any adverse effect on sales growth. The point estimates of the treatment effects are large, but not implausible, particularly given that the confidence intervals include more modest estimates. [42]

Attending the intervention, a course on Corporate Finance, might affect financial policies through different, non-exclusive channels. Participants may learn new corporate finance concepts and methodology from the instructor, they may refresh or

[41]There were 8 listed firms in Mozambique in 2019. Out of these, 6 are non-financial firms and 3 of them participated in our program. Two of these companies where in the treatment group and one of them went public after the intervention.

[42]Bruhn et al. (2019) make a similar argument when measuring the impact of consulting for small- and medium-sized firms in Mexico.

consolidate previous knowledge, they may learn from their peers, or they might generate new business from networking with their classmates. While we cannot formally rule out that networking is driving the results, we do not believe that there is strong support for the networking channel. While our results on ROA are consistent with a network channel, it is less obvious why networking should decrease working capital for the average treated company. Moreover, we would expect to see a positive effect on sales growth if networking translated into new business among treatment firms. Last, during the delivery of the course for the treatment group in May 2017, we organized a networking event for the control group, allowing this group to network as well. Though the duration of this event was admittedly shorter than the duration of the course, this gave control group participants the opportunity to know each-other and exchange contacts.

Our RCT setting overcomes several of the most common identification issues in the literature that tries to establish a relation between the characteristics of managers and firm-level outcomes (it generates exogenous variation amongst participating firms). However, the internal validity of our research design could still be compromised by systematic differences in treatment and control groups due to the small sample, high or uneven attrition rates, contagion effects, or changes in the expectations or behavior of treated managers due to being part of an experiment. We perform differences-on-the-mean and -median tests to compare the two groups and find that almost all differences at the mean and especially at the median are not statistically significant. Moreover, for the validity of our experiment, it would be alright if the groups differed in levels but exhibited parallel pre-treatment evolution. We test the parallel trends assumption non-parametrically and do not find evidence on its violation for the main variables of interest. With respect to non-compliance, our programme participation is very high:

the compliance rate is 91%, with only 4 firms dropping out of the course.[43] We also repeat all our tests on the population of firms that were assigned to treatment and estimate intention-to-treat effects (ITT). The results are very similar. Managers of treated companies might also change their behaviour and expectations differently from the control firms because of the intervention. For instance, managers might update their expectations with respect to firm performance and respond accordingly (see Chemla and Hennessy (2019)). In our setup this is less of a problem as both treatment and control managers expect to receive treatment. Another concern is that they might engage in specific behaviour such as engaging in earnings management, because they want to impress or please instructor/researchers or because they feel they are under scrutiny. This can also get reflected in compliance to share financial data with the researchers. While all firms signed an agreement to share data, we were not able to collect data from all the companies. This is potentially problematic if the willingness to share the data for the treatment groups is different from the control group. By offering the course to both treated and control firms we attempt to mitigate these potential differences, as this should better align the behaviour of treated and control firms. Moreover, we make use of external data, that is not self-reported to us, and show similar treatment effects, addressing the concern that strategic data disclosure is biasing our estimates.

We perform several additional robustness checks to our baseline results. In specific, we use different estimation windows by varying the sample period before the intervention, we exclude firms that had CEO turnovers after the treatment, we restrict the analysis to data obtained from KPMG, we focus on the largest segments of each business group, we use alternative definitions of financial ratios to measure working capital and accounting

[43]For the 41 treated firms that took up the course, class attendance at the manager level was high at 92%, with 72% of the managers attending all of the classes. Class attendance aggregated at the firm level was higher at 93%, with 85% of the firms (at least one participant per firm) attending all the classes. High attendance can be explained by the requirement to attend at least 75% of the classes to receive a certificate of participation.

performance, and we exclude firms that operate in the financial industry.[44]

Whether our findings can be generalized to other contexts in or outside Mozambique is difficult to answer. However, we provide several tests that support the external validity of our findings. When we compare firms that applied to our program with other firms that are present in the KPMG reports, we do not find firms that enrolled in the program to be significantly different from the non-enrolling firms on observable firm characteristics. We also compare the characteristics of participating executives with executives of firms in Mozambique that are present on the LinkedIn network and find no significant differences in tenure or level of education. More interestingly, we also compare our participants with the sample in Graham and Harvey (2001) for firms of similar size (according to revenues) and find not significant differences in tenure or education.

Overall, our results show that financial expertise of managers has a positive and economically significant impact on firm performance through the adoption of financial practices that promote value creation. Moreover, our results suggest that relatively small interventions such as financial education improve financial practices and decision-making and may ultimately affect economic development. Using the median participating firm as a benchmark, we estimate an average impact from the intervention of 1.28 million USD; this estimate is considerably smaller, at about 30,000 USD when we are very conservative and use the lower bound of the confidence interval of the treatment effect. In any case, the treatment effects seem to exceed the direct expected costs of participating in such a programme which are estimated at around 10,000 USD, taking into account the tuition fees.

Given the large positive impacts, why had firms and managers not already taken up

[44]Banks were initially excluded from our experiment, but we still kept firms with other operations in the financial industry (e.g., insurance).

a finance course? There are several non-mutually exclusive potential reasons for this. First, there are no similar courses available locally, which significantly raises the total cost of participating in such a program (incl. traveling and opportunity costs). Second, firms might simply not be aware of the benefits of such an executive training. Kremer et al. (2019) argue that this behavior can also be consistent with behavioral biases by managers of firms in developing countries, such as an inattention or an underestimation of returns or an overestimation of the risks involved, for instance.

The remainder of this paper is structured as follows: the next section gives an overview on financial education and financial practices of firms in Mozambique. In section 2.3, we present the experimental design, describe the executive education programme (intervention) and the data collection process. Section 2.4 shows the results of our intervention based on survey and accounting data. In Section 2.5, we interpret the findings and make some policy recommendations. Section 2.6 concludes.

2.2 Financial Education and Financial Policies of Medium and Large Enterprises in Mozambique

This section motivates our decision to conduct the experiment in Mozambique and explains the selection of firms considered for the experiment. It also describes the design and outcome of an explorative stage in which we collected information on the background (including financial education and experience) of CEOs as well as on firms' current financial practices. The results of this explorative stage were helpful for several reasons. First, there is no data on financial expertise and financial policy available for a large set of firms in Mozambique. Understanding the status quo, in terms of CEO backgrounds and current finance practices, as well as learning more about the

functioning of the financial markets was important to design a meaningful course for that target audience. Second, it helped us to understand whether there was an interest in participating in an "executive education" programme in finance, and what content could be relevant for Mozambique. Last, it allowed us to compare financial expertise and practices of these firms with evidence of firms of similar size and sectors from the US.

2.2.1 Mozambique and the Selection of Firms for the Experiment

We have chosen Mozambique to conduct the RCT for several reasons. First, we expected to observe more heterogeneity in terms of financial education among executive managers when compared to managers of US-American or European firms due to the lack of executive education programmes in finance available in the country.[45] This heterogeneity might be helpful when measuring effects of financial education on financial policies and firm performance. Second, survey statistics collected by the World Bank Enterprise Surveys (2018) suggest that Mozambique is representative of other Sub-Saharan (SSA) economies. Some details of this survey are summarized in Section B1 in the Appendix. Third, Mozambique has an important advantage for the implementation stage: most large companies' headquarters are located in the capital, Maputo. This helps with the logistics and organization of the intervention, and was expected to increase participation rates. Last, we benefited from the existing links between NOVAFRICA, a knowledge center by Nova School of Business and Economics, and governmental organizations as well as NGOs in Mozambique that helped to increase visibility and credibility of the project.

[45]For instance, there is only one business school providing an MBA programme on a regular basis (in cooperation with a Portuguese business school).

We focused the intervention on medium and large firms because they control a large fraction of assets in the economy. Potential efficiency gains of these firms are, therefore, more likely to be economically relevant. Moreover, some capital allocation inefficiencies previously documented in the literature are mostly relevant for large and multi-divisional firms. For instance, Krüger et al. (2015) show that firms do not properly adjust for risk in their capital budgeting decisions, and that conglomerates underinvest (overinvest) in relatively safe (risky) divisions.

In the long-run, there might be also some spillovers of best financial practices from large firms to smaller ones. First, large firms might be role models for smaller firms and those firms may adopt some of the practices of large corporations. Second, there might be some direct knowledge / practice spillovers originating from human capital that is moving across companies. Both channels are likely to be more prominent in large firms.

In addition, financial literacy has been mostly studied in the context of small enterprises (e.g., Drexler, Fischer, and Schoar, 2014), but little is known at the level of large corporations beyond the fact that there is a correlation between financial expertise and financial polices (Custodio and Metzger, 2014; Güner et al., 2008).

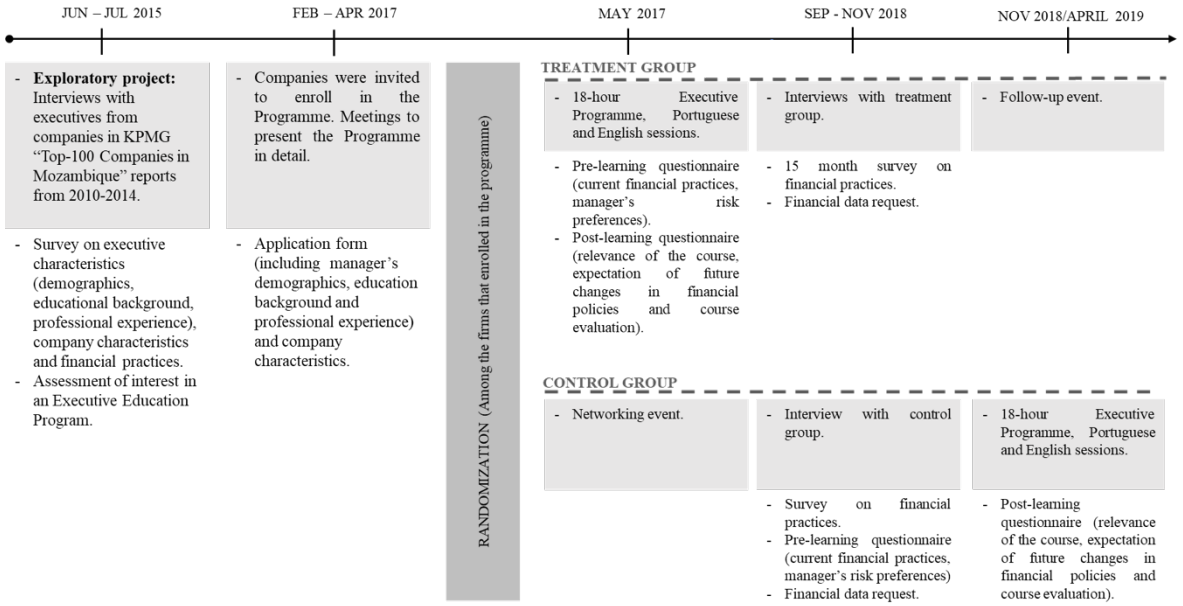
2.2.2 Financial Practices of Firms

An explorative stage of the project took place in 2015 where we collected information about managers, including demographics and financial education and experience, as well as firms' characteristics and financial policies. We used this information to design the executive education programme and to compare financial expertise and finance policies of medium and large enterprises in Mozambique with US evidence.

The exploratory stage run between June and July 2015 (see Figure 2.1). During this period, we contacted 218 companies obtained from KPMG "Top-100 Companies in

Figure 2.1: Project timeline

This timeline describes the field work between June 2015 and April 2019. For each stage, it describes the work performed as well as information collected regarding companies and managers.



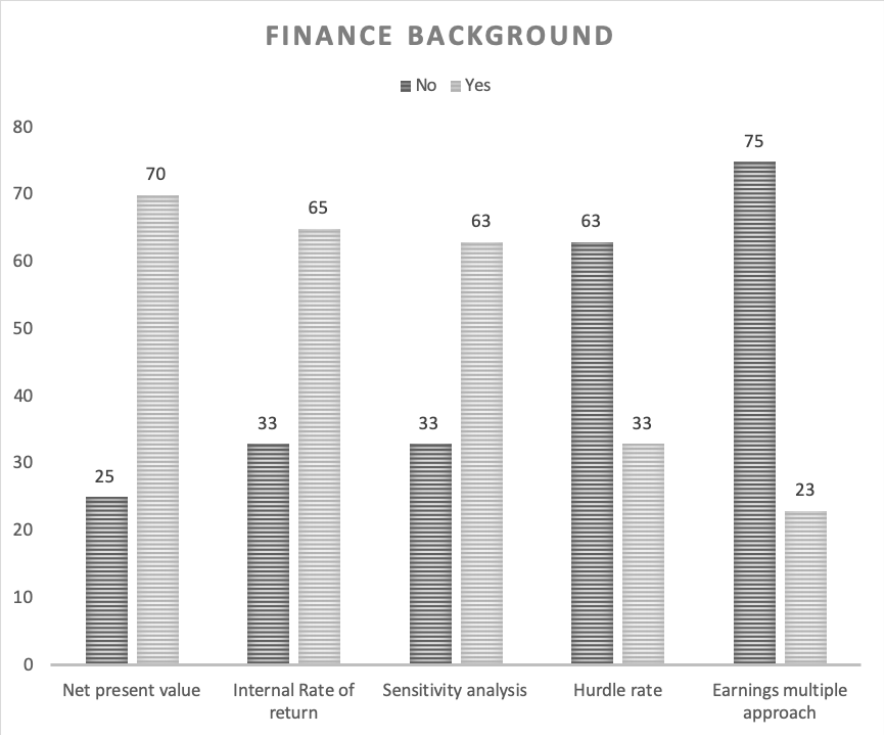
Mozambique" reports from 2010-2014 and had 65 meetings with executives. Out of the 65 meetings, we were able to collect 63 questionnaires.[46] The questionnaires were filled in during a 30-minute face-to-face interview. The interview was conducted at company's premises by a member of the research team. Although we specifically invited the CEO, sometimes our request was forwarded to the CFO, to a member of the accounting team, or in a few cases, to a non-finance related staff.

These questionnaires surveyed financial practices, manager characteristics and overall business aspects of the companies, following Graham and Harvey (2001, 2002). We also used the survey to assess the interest of managers for a free of charge Executive Programme on Financial Management. We specifically asked which topics they would find more relevant, including capital budgeting, risk management, capital structure,

[46]Two participants were busy at the scheduled time and committed to send us the questionnaire later by e-mail, which did not happen. These 63 pilot questionnaires correspond to 62 business groups (in this case single companies), as we surveyed separately two managers from the same company.

Figure 2.2: Financial Experience and Financial Policies

This graph displays the percentage of managers using different valuation techniques according to financial experience. Financial experience is defined as previous background in finance, i.e. managers who have attended at least one finance course at any higher education degree. Source: Survey Jun-Jul 2015.



pay-out policy and mergers and acquisitions. Finally, we inquired the executives' time availability for such a programme and about the optimal schedule to follow it out in order to maximize attendance.

These questionnaires also allowed to have a first look at financial expertise, financial policies, and the interaction between the two in Mozambique. We start by documenting substantial heterogeneity in financial expertise by CEOs in Mozambique. About 43% of the CEOs have a background in finance, either by education or work experience. When analyzing financial practices in firms with and without financial expert CEOs, we find large differences in their practices. For example, Figure 2.2 shows financial practices related to capital budgeting/valuation by firms that are run by financial expert CEOs compared to non-financial expert CEOs. While a large majority of CEOs with a

background in finance is making use of sophisticated valuation techniques such as net present value (NPV) (70%), or conducts sensitivity analysis (63%), this is relatively uncommon for CEOs without such a background. Only 25% of CEOs with no financial background use NPV, and only 33% of them perform sensitivity analyses in their capital budgeting calculations. At the same time, they are more likely to use less sophisticated valuation techniques such as hurdle rates (63%). These findings are consistent with US evidence by Bertrand and Schoar (2003) and Custodio and Metzger (2014) who found CEOs with MBAs or financial expertise are much more likely to follow financial theory and textbook rules, and to avoid common mistakes such as using a unique firm cost of capital irrespective of the nature of the project (the WACC fallacy).

2.3 Design and Implementation of the Experiment

This section presents the experimental design and sample description. It then details the contents of the programme as well as its implementation. Finally, we discuss the data collection procedure.

2.3.1 Experimental Design and Sample Description

Our experimental design is motivated by two common challenges faced by researchers when analyzing the effect of financial education on financial policies: i) the endogenous decision to obtain financial education and ii) limited availability of data.

While the documented correlations between the financial expertise of CEOs and their financial practices in the previous section are consistent with the view that CEO education affects financial policies, a clean interpretation of these correlations remains difficult. Researchers have examined whether corporate outcomes are affected by CEO

characteristics, but no consensus has been reached (e.g., see Chemmanur and Simonyan (2017) for a survey of the literature). CEOs and firms are not randomly matched, and there is the concern that endogenous matching biases the estimates. Indeed, the literature on effects of managerial human capital on firm policies heavily relies on cross-sectional analysis that makes causal inference very challenging. Some studies such as Bertrand and Schoar (2003) use panel regressions and estimate potential CEO effects making use of within-firm variation due to CEOs switching firms. However, Fee et al. (2013) cast doubt on this methodology for identifying managerial style effects on policy choices. They argue that CEO turnover events are endogenous and managerial "style changes" are anticipated by corporate boards at the time of the CEO selection decision. In other words, while firm-fixed effects allow to control for unobserved firm heterogeneity that is time-invariant, it cannot be ruled out that firm time-varying characteristics, unobserved by the econometrician such as some strategic decisions, drive both financial policies and the characteristic of the CEO that is appointed. In the context of financial expertise, Custodio and Metzger (2014) show that firms run by managers that have past work experience in finance have better access to external financing and allocate the firms' financial resources more efficiently. At the same time, however, they also provide evidence that financial expert CEOs are more likely to be appointed by more mature firms.

In order to identify a treatment effect of financial expertise on firm policies, one would need to randomize financial expertise across firms. One way of doing so would be an actual random allocation of CEOs to firms. Unfortunately, this type of experiment is not feasible in the context of large firms. We propose a different solution, by randomizing financial education of top managers, and, at the same time, keeping the match between CEOs and firms constant. To be specific, we "treat" managers with financial education by offering free MBA-style lectures on corporate finance and risk management to top

managers. Such a randomized controlled trial (RCT) can be used to identify a treatment effect of finance education on financial policies.

A second challenge for our study is the availability of data. First, most companies are private and the access to financial statements is limited. Moreover, some outcomes, such as the use of specific valuation techniques or risk management instruments, are difficult to measure in those statements. For that reason, we provide incentives to firms to share financial statements with us and complement those statements with survey data from interviews, allowing us to collect non-standard data. For a large set of firms, we complement this data with accounting information directly from external reports, "Top-100 Companies in Mozambique" published annually by KPMG Mozambique.[47] The last piece of data allows us to validate the self-reported data and helps to address the concern that some firms might be strategic in their choice of sharing data with us.[48]

We construct the treatment and control group of our experiment in two steps. First, we invited 577 medium and large companies to sign up for an executive education programme on finance. The list of invited companies is primarily composed of companies appearing in a KPMG report at least once in the period 2009-2016 (391 companies). Additionally, we invited companies associated with local business associations, namely CTA – Confederação das Associações Económicas de Moçambique and ACIS (186 companies).[49] We restrict our sample to companies that are headquartered in Maputo.[50]

[47]These reports contain name and information of many of the largest corporations in Mozambique. These reports are publicly available and are used by local and foreign investors, public administration and other institutions. Each report lists and ranks the 100 largest companies (according to total revenue) from the pool of companies that fill-in the KPMG annual survey. For each company, it provides main financial accounting figures such as revenues, net income, assets, liabilities, equity, number of employees and new investments.

[48]We discuss this concern in more detail in Section 2.4.5 of the paper.

[49]We partnered with these two business associations as their work receives national recognition. This fact contributed to raise public awareness about our project.

[50]Sutton (2014) presents detailed profiles of 40 Mozambican companies, chosen to represent the leading firms in several industries. Out of those 40 companies, 24 appear in our set of invited companies. The match is much larger when we exclude companies from extractive industries (the ones located in specific regions of the country and usually outside Maputo). Out of 19 remaining firms, 16 were invited

This enabled in-person interaction with participants, which was crucial throughout the project to engage the participants with the programme and facilitate data collection. This requirement also reduced non-compliance of participants as it minimized the participants' cost of attending the training. We focussed on executives of these companies as those usually take most strategic decisions, including financial decisions (see Harvey et al. (2015)).

To address the concern of endogenous selection into the treatment, we randomly allocated firms that enrolled in the programme into two groups: the treatment group and the control group. The randomization was done within industry to make sure the same industries were represented in both groups.[51] We then offered the treatment, the course on finance, to the two groups in a staggered way. The first cohort - the treatment group - received the treatment in May 2017, while the second cohort - the control group - received the same treatment in November 2018/April 2019.[52] The reasoning for teaching the control group as well has the following rationale: First, it incentivizes the control group to share their accounting data and participate in the surveys as well. Second, it helps addressing the concern that the formation of expectations may bias the experiment (Chemla and Hennessy, 2019) because treatment and control group both expect to be treated.

The advertised course was an Executive-level Programme in Finance - "Finance and

to take part in our project.

[51]The randomization procedure was done at company level and stratified by industry. As noted by Sutton (2014), a sample stratified by industry provides a 'fair and complete picture of the country's industrial capabilities'. However, there were several business groups in our sample (i.e., one manager might oversee several companies belonging to the same group). Given that the intervention is at manager level, we could not allow for treatment and control companies within a given manager. Therefore, after an initial random assignment on the pool of companies, we observed the assignment of the most relevant company (according to size) in each business group and extend that assignment to all companies within manager.

[52]While not affecting the internal validity of the experiment, it is still interesting to analyze the characteristics of firms and executives who are interested in attending the course versus who are not. Please see also Section B2 in the appendix of the paper.

Strategy – Value Creation in Emerging Markets" - promoted under Imperial College Executive Education branding. The course was offered in Maputo, free of charge and limited to the companies participating in the research project. Additional information about the course was openly available at the Imperial College Executive Education webpage, including a market price of £6,500 per participant / free of charge for invited participants.[53]

Upon receiving 109 positive responses, we scheduled face-to-face meetings to present further details about the programme. Managers that were interested in the programme formalized their interest on behalf of the company by submitting an application form. This form collected information on manager characteristics (demographics, educational background and professional experience) and company characteristics. The registration form also contained a data access agreement for the provision of financial information (income statement and balance sheet). We allowed each company to send up to two attendees imposing that at least one of them was a senior manager.[54] We received application forms from 111 participants, corresponding to 93 firms. Those companies were then randomly allocated into treatment (45 companies) and control group (48 companies) two weeks before the first intervention. We made sure companies that were part of the same business group were allocated to the same cohort. 46 managers effectively participated in the programme, representing 41 companies and 31 business groups (Table 2.1).

Table 2.2 panel A shows summary statistics for the participating firms (treatment and control groups) and differences between the two groups, in the year before the intervention. The average treated firm has total assets of 22.3 million USD, total revenue of 15.8 million USD, and 191 employees. The distributions are very skewed,

[53]See the webpage and brochure in Appendix B4 of the paper.
[54]We required one application form per attendee.

Table 2.1: Number of managers and companies participating in the programme

The table displays the number of participating companies and managers at different stages of the project.

Time	What	Firms	Managers
Pre-Treatment	**Invitations and applications to the programme; randomization**		
	Companies that applied to the programme	93	-
	- Treated companies	45	-
	- Control companies	48	-
	Financial data		
	- Treated companies	36	-
	- Control companies	42	-
Treatment (2017)	**Intervention I**		
	- Programme attendees	41	46
	- Control event attendees	18	17
Post-Treatment	**15month survey**		
	- Treated companies	30	22
	- Control companies	39	31
	Financial data		
	- Treated companies	32	-
	- Control companies	35	-

and, by chance, there are two large firms in the control group, resulting in higher means of size related variables in the control group (significant at the 10 percent level). When we compare financial ratios or the medians, both differences between the two samples are much smaller.[55]

Panel B of Table 2.2 shows summary statistics for the top managers in the treatment and control groups, as well as the differences across the two groups. About 61% of the managers in the treatment groups are the CEO of the company and 29% the CFO. These managers are in general highly educated, with 57% having a masters degree or higher. A large fraction also has a finance or accounting related education, with only 19% of them reporting no education in finance or accounting at any level. About 19% of the executives are female. Differences across the two groups are not statistically significant. The only exception is nationality. About 55% of the managers in the treatment group are Mozambican, compared to 78% in the control group.

[55]Appendix table B9 describes variables construction and respective sources.

Table 2.2: Base line summary statistics

Panel A displays summary statistics for the main financial variables of 93 firms participating in the programme (Treated/Control Sample). Financial data is obtained from KPMG "Top-100 Companies in Mozambique" report, and hand collected. Panel B displays demographic, educational and professional characteristics of managers reported in the application forms for treatment and control firms. The category "Masters or higher" contains the "MBA" category. Top manager is defined as the most senior participant filling in the application form for a given business group. The (descending) order of seniority considered is CEO, CFO, accountant or related, other directors or staff and sales manager or related. When more than one manager had a top position due to turnover during the project, we considered the manager with the longest reported tenure. All values are reported as of 2016. *, **, *** Significance at 10, 5 and 1%, respectively.

Panel A: Treatment/Control companies

	Treatment				Control				Mean		Median	
	Obs.	**Mean**	**Median**	**St.Dev.**	**Obs.**	**Mean**	**Median**	**St.Dev.**	**Diff.**	**p-value**	**Diff.**	**p-value**
Total Assets (m USD)	34	22.29	4.60	47.60	42	126.33	9.86	339.06	-104.04	0.08*	-5.26	0.49
Sales (m USD)	34	15.84	3.12	38.94	42	58.94	8.32	132.9	-43.1	0.07*	-5.20	0.11
Sales Growth	34	-0.35	-0.36	0.23	38	-0.14	-0.27	0.76	-0.21	0.12	-0.09	0.10
# Employees	32	191.06	81.5	248.63	38	308.26	102.5	541.02	-117	0.26	-21	0.81
Cash / Assets	19	0.10	0.06	0.10	26	0.17	0.11	0.21	-0.07	0.21	-0.05	0.09*
Leverage	25	0.15	0.10	0.20	32	0.26	0.11	0.37	-0.11	0.2	-0.01	0.91
Capex / Assets	23	-0.01	0.00	0.11	36	0.06	0.01	0.33	-0.07	0.33	-0.01	0.92
Return on Assets (ROA)	33	0.12	0.05	0.27	41	0.05	0.07	0.22	0.07	0.25	-0.02	0.64
Return on Inv. Cap. (ROIC)	32	-0.04	0.04	1.37	41	0.74	0.18	2.23	-0.78	0.09*	-0.14	0.28
Working Capital / (Lag) Assets	24	0.04	-0.05	0.41	38	0.15	0.17	0.31	-0.11	0.23	-0.22	0.19
Working Capital/ Sales	24	0.13	-0.03	0.91	38	0.12	0.14	0.59	0.01	0.96	-0.17	0.19
Inventories / Sales	18	0.13	0.05	0.23	25	0.16	0.03	0.24	-0.03	0.73	0.02	0.66
A/R / Sales	19	0.36	0.27	0.32	26	0.31	0.23	0.28	0.05	0.59	0.04	0.9
A/P / Sales	18	0.32	0.12	0.41	26	0.23	0.12	0.26	0.09	0.39	0.00	0.76
Avg. Collection Period	19	131.68	100.04	116.91	26	113.55	84.79	103.94	18	0.59	15	0.9
Panel B: Treatment/Control Managers (Top Manager)												
Male	31	0.81	1.00	0.40	36	0.75	1.00	0.44	0.06	0.59		
Age (years)	30	43.73	41.00	7.96	34	45.26	43.50	10.71	-1.53	0.52	-2.50	0.43
Tenure (years)	31	7.28	5.00	5.77	35	7.60	5.00	7.23	-0.32	0.84	0.00	0.84
Mozambican	31	0.55	1.00	0.51	36	0.78	1.00	0.42	-0.23	0.05*		
CEO	31	0.61	1.00	0.50	36	0.53	1.00	0.51	0.09	0.36		
CFO	31	0.29	0.00	0.46	36	0.31	0.00	0.47	-0.02	0.89		
Masters or higher	30	0.57	1.00	0.50	33	0.42	0.00	0.5	0.15	0.27		
MBA	30	0.20	0.00	0.41	33	0.21	0.00	0.42	-0.01	0.91		
Acc. or finance background	27	0.81	1.00	0.40	33	0.73	1.00	0.45	0.08	0.43		

Even though the experimental setup helps to identify a causal effect of financial executive education on firm outcomes, there are still limitations. For instance, attendance of the course is voluntary and (failing of) sharing of the data might be non-random. Moreover, the mechanisms through which a finance executive education course impacts firm outcomes may be wider than the learning channel itself. Indeed, the classroom experience may affect dimensions that are not directly related to the content of the course. Managers may benefit from networking with managers of other firms while attending the course. If they start doing business together, it can eventually translate into higher revenues, even though it is unrelated to learning. In order to alleviate some of the potentially confounding effects of networking, we organized a networking event for companies in the control group. This event took place around the dates of the first intervention, i.e. when the treatment group attended the course. We discuss the networking event as well as some other threats to the internal validity and the interpretation of our findings in detail in Section 2.4.5 of the paper.

2.3.2 Design of the Course

The course was designed as a general course in corporate finance, but emphasized topics identified as weaknesses by the managers at the explorative stage (see Section 2.2.2). We also used the survey and face-to-face interviews conducted during the explorative stage to evaluate the trade-off between the executives' willingness to participate and the course content and duration to maximize participation. The proposed course contains standard topics of any corporate finance course (i.e., capital budgeting, valuation, and capital structure) plus modules on working capital and risk management. The course consisted of four modules:

1. **Capital Budgeting and Valuation**: This module covered standard techniques

of firm and project valuation such as discounted cash flows methods, net present value, internal rate of return, payback period. It also covered asset pricing models such as CAPM as a tool to estimate project discount rates. Some common valuation mistakes such as the misuse of the weighted average cost of capital irrespective of the specific risk of the project were also covered in the course.

2. **Capital Structure**: This module presented a practical view of assessing the optimal capital structure of the firm, listing the advantages and disadvantages of debt financing such as the tax shield of debt and bankruptcy costs, respectively.

3. **Managing Working Capital**: This topic covered the concept of working capital and the impact of efficient working capital management on cash flows and cash holdings. This module also covered cash management, and management of account receivables and account payables. For instance, participants were taught how to calculate the cost of trade credit and compare it to other sources of financing.

4. **Risk Management**: This module covered the identification of risks and associated potential costs, analysis of the causes of risk of financial loss, determination of various hedging strategies, implementation of the risk management strategies, and management and monitoring of results. The approach to this topic was that an effective risk management programme can reduce losses and improve financial performance.

The intended learning outcomes of these four modules can be summarized as follows:

1. Read, understand and process (for instance calculate basic financial ratios) financial information from financial reports.

2. Understand the impact of efficient working capital management on firm liquidity and funding needs.

3. Understand the appropriate valuation techniques to use when making capital budgeting decisions, and avoid common mistakes in valuation, for instance do not take the time value of money into account.

4. Trade-off the costs and benefits of a given financial structure and source of financing.

5. Identify sources of risk and risk management practices, for instance hedging using insurance or financial instruments.

The course was organized in four modules spanning 18 hours (4.5 hours each). While this may appear relatively short, courses in related studies have similar durations (e.g., two days or two half days (Bruhn and Zia, 2013; Field et al., 2010)). Moreover, our course is at the shorter end but in line with sessions on related topics in typical MBA core courses in corporate finance. Given that participants were top executives, our survey results also suggested that many CEOs/CFOs found it difficult to accommodate longer duration courses in their agendas. By keeping the intervention short, we may have increased participation, potentially at the expense of the intensity of the intervention.[56] At the same time, shorter courses are cheaper and simpler to organize from a logistical point of view, a potentially important criterion from a policy point of view.

The format of the course was a mixture of lectures and case studies. The case studies illustrated the different topics in a relevant setting for larger firms operating in emerging markets. For instance, we made use of the following Harvard Business School case studies: New Earth Mining (evaluating a new investment opportunity in South Africa), Mozal (large investment project in Mozambique), Supply Chain Finance at Procter and Gamble and Fibria (working capital management and its liquidity consequences in the US and Brazil). The course was delivered both in Portuguese and English (the group

[56]It might be interesting to experiment with the length of such a course in future extensions of this project.

was split according to their language preferences), by the same instructor. Participants who attended a minimum of 75% of the classes received a participation certificate from Imperial College Business School.

2.3.3 Delivery of the Course and Data Collection

2.3.3.1 Intervention 1 - Delivery of the Course for Cohort 1 (Treatment Group) and Networking Event for Cohort 2 (Control Group)

The first edition of the course took place in May 2017. Out of the 45 treatment firms, 41 showed up and stayed for the full duration of the course (participation rate of 91%).[57] Figure 2.3 reports the number of companies participating in different stages of the project.

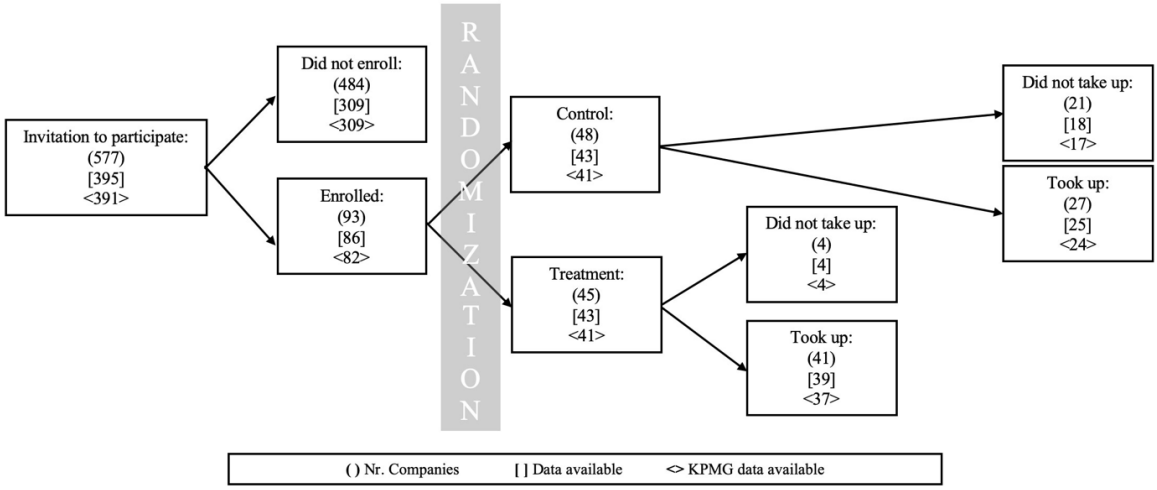
First, the participants were required to fill-in a pre-learning survey. This survey replicated the exploratory project survey and collected baseline information on current financial practices of the company. At the end of the course, participants filled in a post-learning exit survey. The post-learning exit survey was divided into a confidential part, where participants were asked to evaluate the course, and a non-confidential part, where they described their intentions to change financial practices in future.

A concern that remains is that there are confounding effects related to the treatment. In particular, network effects instead of the content of the course itself may lead to changes in some outcomes of interest. While these potential network effects are less obvious for financial policies, we are more concerned with them affecting profitability.

[57]Four companies did not adhere to the randomized protocol. Two of them enrolled through email/phone and promised to deliver the application form later on. We were not able to reach them later. The other two enrolled and confirmed attendance in the first edition but did not show up on the day of the course. After a follow-up call, one manager stated he was away due to an unexpected meeting in Angola, whereas another firm was experiencing an internal re-structuration that required manager's presence.

Figure 2.3: Number of companies in different stages of the experiment

This diagram shows the number of companies participating in each stage of the experiment (round brackets). It also reports the number of companies for which we have at least one year of financial data either from KPMG or self-reported data (square brackets) or from KPMG (angle brackets).



Profitability is a critical outcome to understand whether potential changes in financial policies lead to more efficient outcomes. Networks can affect profitability in several ways: attendees may form new business relationships or share relevant information or knowledge. To address this concern, we organized an afternoon networking event for the control group, whose purpose was to give control group the opportunity to mingle and to network. This event featured a short presentation of the Executive Education programme as well as short interventions from public and private-sector invited keynote speakers.[58]

2.3.3.2 Intervention 2 - Delivery of the Course for Cohort 2 (Control Group)

Between September and November 2018, we contacted and visited companies in the control group (40 meetings out of 48 control companies). In these meetings, we run

[58]Importantly, the network event did not overlap with the content of the course.

the pre-learning questionnaire (similar to the one applied in the treatment group). Moreover, we also asked which financial practices had changed since May 2017 and investigated expectations regarding future changes. This survey was intended to provide a counterfactual for implemented changes in financial practices by the treatment group. In these interviews, we also requested financial accounting data.

In a few cases, the manager that had applied to the programme was replaced. For these cases, we briefed the new manager about the programme and invited her or him to participate in the second intervention. The second cohort of the course was taught in November 2018 (in Portuguese) and in April 2019 (in English). The course's content and teaching method was replicated from the first edition. At the end of the course, participants were required to fill-in the post-learning exit survey as described in the previous subsection.

Out of 48 control companies, 27 showed up on the days of the course (participation rate of 56%).

2.3.3.3 Follow-up survey and Financial Reports

Approximately 15 months after the first intervention, between September 2018 and November 2018, we surveyed managers in the treatment group. We asked them about implemented changes with respect to financial policies since the first intervention. We also requested their financial reports data between 2013 and 2018. We provided companies with a template spreadsheet including balance sheet, income statement and statement of cash flows items to fill in.

We also collected financial information for the period between 2008 and 2018 from the report "Top-100 Companies in Mozambique" published annually by KPMG Mozambique. We use this data to add financial information of companies that did not or

could not share their financial data, and to assess the quality and consistency of the data provided by the companies through the Excel spreadsheets. This information was collected for both treatment and control firms. Financial data was available in dollars and/or Metical depending on the source. We converted all values in Metical to Dollars using the exchange rate of the reporting date. Out of 93 participating companies, we were able to obtain at least one year of financial data for 86 companies.

2.4 The Effect of Financial Education on Financial Policies and Efficiency

This section analyses the effect of financial education on financial policies. We measure the intentions of treated firms to implement changes of financial policies after the courses in May 2017 and November 2018 / April 2019, respectively. We also compare implemented changes of financial policies of firms taught in May 2017 (treated firms) and firms yet to be treated (control firms) in September-October 2018, i.e., before the delivery of the course to the control group. We use both survey evidence and accounting data to measure the outcomes of interest.

2.4.1 Intention to Change Financial Policies (Exit Survey)

We start our analysis by evaluating the intentions of treated firms to change financial policies. We focus on valuation techniques, working capital management, capital structure, and risk management, the main themes of the delivered courses. Table 2.3 shows the results of the exit survey by the participants at the end of the courses.

Panel A of Table 2.3 presents the results for the first cohort that was treated in May

Table 2.3: Intention to change Financial Policies (Exit Survey)

The table displays the intentions of managers to change corporate policies. The data was collected in the exit survey at the end of the course. "N/A" means that a corporate policy cannot be changed because firm does not have discretion over that policy (e.g., subsidiary of a foreign firm). "Miss." refers to a missing answer. Depending on the specification, we disregard this answer in the aggregation or, being conservative, interpret it as a "No". The left tables show the raw answers of the individual managers. Source: Exit survey of cohort 1 (May 2017), Exit survey of cohort 2 (November 2018, April 2019).

Panel A: Cohort 1 (May 2017)

	Intention to implement changes in corporate policies						
	Yes	**No**	**N/A**	**Miss.**	**#**	**% Yes**	**% Yes (incl. missing, excl. N/A)**
Working capital	27	7	3	3	40	73%	73%
Risk management	23	6	7	4	40	64%	70%
Valuation	14	12	7	7	40	42%	42%
Capital structure	13	8	13	6	40	38%	48%

Panel B: Pooled cohorts 1 & 2 (May 2017, November 2018, April 2019)

	Intention to implement changes in corporate policies						
Working capital	44	14	4	6	68	71%	69%
Risk management	40	15	8	5	68	63%	67%
Valuation	30	19	8	11	68	53%	50%
Capital structure	27	18	16	7	68	44%	52%

2017 (treatment group). The survey reveals several interesting findings. i) There is large heterogeneity in terms of firms' ability to implement changes across different policies. "N/A" denotes cases when firms argue that they cannot adjust a particular policy. Capital structure appears to be the policy where managers have the least discretion over. Almost 40% of the companies (13 out of 34) say that they cannot change the capital structure themselves. Survey questions that aimed to understand the origins of those constraints suggest that some companies are subsidiaries of larger (often international firms) and do not have the flexibility to set their own capital structure. ii) Managers aim to implement changes in all financial policies. Among firms that have the discretion to set their own policies and we disregard cases when managers did not answer a question ("missing"), between 38% and 73% intend to implement changes in their policies that were discussed in the course. When we treat missing answers as

"no", the corresponding numbers are between 48% and 73%. iii) Depending on the policy, there is substantial heterogeneity in the intention intensity. Working capital management and risk management are the policies that managers are most likely to change (73% and 70%). There are fewer intended changes of capital structure and valuation techniques (48% and 42%, respectively).

Panel B shows corresponding results when we include answers of the second cohort that was treated in November 2018 / April 2019. While there are some minor differences in the level, the qualitative picture remains robust. Overall, the exit surveys provide strong evidence that firms intend to change their financial policies after the treatment.

2.4.2 Changes of Financial Policies (15-months Survey)

Even though firms express their intentions to change several corporate policies, it remains unclear to what extent they (are able to) implement those changes. To shed light on actual implementation we surveyed participating companies, i.e., treatment and control firms, about 15 months after the first intervention and before the second intervention. There are indeed reasons why firms may end up not implementing intended changes. For example, firms may not have the resources or the personnel to do so, there might be other items on the agenda with higher priorities, external conditions might impose constraints, etc. Moreover, there could be reasons unrelated to the treatment that lead firms to change their policies. To better understand the effect of the treatment itself we explicitly asked firms whether they changed firm polices because of the course. More importantly, we also survey the population of control firms. This allows us to compare changes in financial polices between treatment and control firms as well.

Table 2.4 shows the results. First, between 7.7% and 30.8% of the firms mention that

they have implemented changes of financial policies in the preceding 15 months. Not unexpectedly, implementations rates are much smaller compared to the intentions that were reported in the exit survey. Consistent with the exit survey, working capital management is the most affected policy (about one third of treated companies state that they have implemented changes in their working capital management). Capital structure and valuation techniques are relatively less revised, consistently across the two surveys. One exception is risk management that ranked very high on the list at the exit survey but only very few companies (two companies) state that have implemented changes 15 months later. In the survey, we also asked for reasons which prevented firms from implementing planned changes. One main reason for not changing risk management practices appears to be the limited supply of hedging instruments / products to the Mozambique market. Second, analyzing the motivation for implementation changes in financial policies, firms seem to respond to the treatment. Almost all firms that reported that they implemented changes in financial policies declared that they did so because of the course (second column of Table 2.4).

While these results are suggestive, we can also make use of the control group to address the concern that we may capture a pure time-effect, for instance. Indeed, it might be the case that changes in the economy may have led companies to change their financial policies, irrespective of the treatment. We conducted the survey for the control group at the same time of the survey for the treatment group, before the second intervention of November 2018 (when the treatment group participated in the course). The middle panel of Table 2.4 shows the corresponding evidence for the control group. Only two firms reported that they have implemented changes related to financial policies (working capital management and valuation) over the preceding 15 months. The right panel of Table 2.4 tests for statistical differences between the means of treatment and control group (using a one-sided t-test). We find a large and significant difference of 27.1

Table 2.4: Changes in Financial Policies after 15 Months (15M Survey)

The table displays the implemented changes of corporate policies by managers 15 months after the first treatment (May 2017) and before the second treatment in November 2018. The data was collected through a survey in Sep-Oct 2018. "N/A" means that a corporate policy cannot be changed because firm does not have discretion over that policy (subsidiary of a foreign firm). Depending on the specification, we disregard this answer in the aggregation or, being conservative, interpret it as a "No". The middle of part of the table shows the corresponding answers by control firms (i.e., firms that participated in the experiment but were not taught in the course in May 2017). The right part of the table shows the difference between treatment and control firms and p-values of the corresponding one-sided t-tests. Source: 15M survey (Sep-Oct 2018). *, **, *** Significance at 10, 5 and 1%, respectively.

After 15 months, have you implemented changes in corporate policies?

	Treatment					**Control**				**Difference**	
	Yes	**Yes (b/c of course)**	**No**	**#**	**%Yes**	**Yes**	**No**	**#**	**%Yes**	**Diff.**	**p-value**
Working capital	8	8	18	26	30.80%	1	26	27	3.70%	27.10%	0.00***
Risk management	2	2	24	26	7.70%	0	27	27	0.00%	7.70%	0.07*
Valuation	3	2	23	26	11.50%	1	26	27	3.70%	7.80%	0.14
Capital structure	3	2	23	26	11.50%	0	27	27	0.00%	11.50%	0.04**

percentage points of firms having implemented changes in working capital management. This difference is significant at the 1%-level. With respect to working capital management, additional open questions in the survey reveal that the main issue that most companies identified for themselves after the course has been long collection periods. Companies aimed to tackle this problem in several ways, e.g., by i) tracking (late) payments in a more systematic manner, ii) shortened terms, or iii) hired additional personal for A/R management. The differences in terms of changes in capital structure, risk management, and valuation techniques are smaller and less significant.

Overall, the comparison of treatment and control group is consistent with the view that attending the course led firms to change certain financial policies, especially those they have discretion over. Moreover, the 15-months survey results are also in line with the intentions by the treated firms to change financial policies during the exit survey, right after the treatment. Implementation rates are, however, lower compared to the intentions.

2.4.3 Changes of Financial Policies (Financial Accounting Data)

While the last two sections make use of survey data by treatment and control firms, we can also measure potential changes of financial policies in their financial reports. The financial statements contain information that allow us to investigate potential changes in working capital management, investment and capital structure. Changes in risk management and valuation techniques are more difficult to measure without survey data. The financial data also allows us to measure potential efficiency effects of the executive education programme.

Table 2.5 reports the estimates of treatment effects on main financial policies using ordinary least squares (OLS) to compare treatment and control firms in the cross section

(specification (1)), and firm fixed effects exploiting within firm variation (specifications (2) to (5)). We control for general changes in the business environment by including year fixed effects in specifications (4) and (5). In the last specification, we add firm size as an additional control. In most regressions, we cluster standard errors at the firm level; standard errors are bootstrapped in specification (3). Overall, the estimated coefficients show little variation across these different specifications.

As suggested by the survey evidence of Sections 4.1. and 4.2., we start our analysis by investigating changes to the working capital (WC) management in Panel A of Table 2.5. The coefficient of interest is the interaction term, corresponding to a difference-in-difference estimate. In columns (1) to (5), we scale WC by lagged assets and in columns (6) to (10), WC is scaled by contemporaneous sales. When we scale WC by lagged assets, we find a point estimate of -0.170 that is significant at 10% level. This corresponds to a negative impact on working capital of 0.49 standard deviations (based on the pooled sample of treatment and control firms). Columns (2)-(5) show firm fixed effect estimates. We find similar, slightly larger coefficients between -0.198 and -0.216. Estimates are statistically significant at the 5% level across firm fixed effects specifications and year dummies. Columns (6)-(10) shows the impact of the treatment on working capital scaled by sales. Consistently, the effects are negative - treated firms decrease their working capital by about 0.44 standard deviations - and significant at the 1% level.

In Panels B and C of Table 2.5, we analyse the different components of Working Capital in more detail. Consistent with evidence from the 15-months survey (see Section 2.4.2), we find large and significant effects on accounts receivables (A/R). The difference-in-difference estimate is about -18%, corresponding to a drop of about 0.60 standard deviations or a reduction of about 60-65 days on the collection period. We do not find any significant effect on accounts payable (A/P) which is consistent with the survey

Table 2.5: Changes in Financial Policies (Financial Data Regressions)

The table displays the difference-in-difference estimator for firm financial outcomes. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

Panel A: Working Capital (WC)

	Working Capital/Assets					**Working Capital/Sales**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	-0.170*	-0.216**	-0.216***	-0.216**	-0.198**	-0.320***	-0.406***	-0.406***	-0.409***	-0.386***
	[0.087]	[0.083]	[0.081]	[0.082]	[0.083]	[0.121]	[0.134]	[0.128]	[0.135]	[0.128]
Treatment	-0.172**					-0.165				
	[0.077]					[0.140]				
Post	0.137**	0.100*	0.100*			0.123**	0.086	0.086		
	[0.059]	[0.056]	[0.056]			[0.057]	[0.065]	[0.062]		
Constant	0.185***					0.218**				
	[0.052]					[0.087]				
Observations	409	409	409	409	409	466	466	466	466	466
R-squared	0.069	0.020	0.020	0.058	0.066	0.033	0.045	0.045	0.067	0.076
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of firm_id		68	68	68	68		71	71	71	71

Panel B: Accounts Receivable and Average Collection Period

	Accounts Receivable					Avg. Collection Period				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	-0.216***	-0.185***	-0.185***	-0.183***	-0.179**	-76.332***	-64.642**	-64.642**	-64.213**	-62.596**
	[0.069]	[0.067]	[0.066]	[0.067]	[0.075]	[25.270]	[24.683]	[25.398]	[24.675]	[27.558]
Treatment	0.070					22.864				
	[0.079]					[26.893]				
Post	0.225***	0.214***	0.214***			82.626***	78.472***	78.472***		
	[0.062]	[0.063]	[0.065]			[22.547]	[22.847]	[24.256]		
Constant	0.246***					89.269***				
	[0.043]					[15.469]				
Observations	212	212	212	212	212	212	212	212	212	212
R-squared	0.052	0.132	0.132	0.181	0.181	0.058	0.153	0.153	0.197	0.198
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of firm_id		45	45	45	45		45	45	45	45

Panel C: Accounts Payable and Inventories

	Accounts Payable					Inventories				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	-0.352	-0.309	-0.309	-0.308	-0.306	-0.149*	-0.159*	-0.159	-0.160*	-0.153*
	[0.291]	[0.275]	[0.269]	[0.277]	[0.277]	[0.077]	[0.088]	[0.103]	[0.089]	[0.087]
Treatment	0.391					0.040				
	[0.314]					[0.070]				
Post	0.124**	0.101**	0.101**			0.093**	0.091**	0.091***		
	[0.048]	[0.044]	[0.045]			[0.038]	[0.037]	[0.033]		
Constant	0.167***					0.097***				
	[0.028]					[0.025]				
Observations	208	208	208	208	208	205	205	205	205	205
R-squared	0.041	0.017	0.017	0.022	0.022	0.009	0.013	0.013	0.058	0.059
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of firm_id		44	44	44	44		43	43	43	43

evidence as well. We can only speculate why firms change A/R but not A/P after the intervention. One potential reason is that firms can more easily change their own terms (with clients), while negotiating longer payment periods with suppliers might be more difficult. Last, we also find a negative effect on inventories. The point estimates range between -0.149 and -0.160 and are statistically different from zero at the 10%-level.

Overall, the results on working capital management suggest that firms respond to the treatment by decreasing the collection period as well as their inventories. This reduction in working capital leads to a cash inflow, potentially affecting other corporate polices beyond a direct effect of the treatment.

Table 2.6 reports the impact of the treatment on other firm policies: leverage, cash holdings and total investment in fixed assets (capex). Panel A shows that the effect of the intervention on the capital structure (leverage and cash holdings) is not statistically significant. This does not necessarily mean that firms do not adjust their capital structure in response to the treatment. Indeed, different companies could react to the treatment by adjusting their leverage, for instance, in different directions given that some companies might be below their optimal leverage level, while other companies were above. However, those findings are also well in line with the survey evidence on capital structure decisions in which only 3 companies stated that they implemented changes. As discussed before, some firms are subsidiaries of larger (often international) corporations and do not have discretion of those policies. They also claim that credit markets in Mozambique are tight and it is very difficult or too expensive to obtain debt.

If companies do not change their capital structure nor their cash holdings in response to the inflow of cash after the reduction of their working capital, it is interesting to investigate how this cash is used. For instance, companies may increase their dividends, use this cash to invest into fixed capital or engage in other expenses. Although we

Table 2.6: Changes in Cash, Leverage and Capital Expenditures (Financial Data Regressions)

The table displays the difference in difference estimator for firm financial outcomes. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

Panel A: Cash and Leverage

	Cash					**Leverage**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	0.032	0.031	0.031	0.033	0.057	-0.138	-0.055	-0.055	-0.055	0.029
	[0.044]	[0.046]	[0.047]	[0.046]	[0.046]	[0.125]	[0.081]	[0.090]	[0.079]	[0.097]
Treatment	-0.107**					-0.120*				
	[0.043]					[0.071]				
Post	0.007	0.000	0.000			0.100	0.051	0.051		
	[0.034]	[0.034]	[0.038]			[0.119]	[0.078]	[0.082]		
Constant	0.215***					0.318***				
	[0.036]					[0.062]				
Observations	201	201	201	201	201	436	436	436	436	436
R-squared	0.064	0.005	0.005	0.123	0.167	0.027	0.002	0.002	0.047	0.117
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of firm_id		45	45	45	45		74	74	74	74

Panel B: Capital Expenditures

	Capital Expenditures				
	(1)	(2)	(3)	(4)	(5)
Treatment x Post	0.134***	0.131***	0.131***	0.126**	0.144**
	[0.049]	[0.049]	[0.045]	[0.049]	[0.055]
Treatment	-0.059**				
	[0.026]				
Post	0.052*	0.061**	0.061**		
	[0.029]	[0.028]	[0.027]		
Constant	0.028				
	[0.022]				
Observations	164	164	164	164	164
R-squared	0.115	0.142	0.142	0.197	0.211
Firm FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes
Number of firm_id		44	44	44	44

do not have payout or granular expense data, we can analyze long term investment (capital expenditures). In Panel B of Table 2.6, we document a positive and significant treatment effect of the course: firms that were part of the treatment group increased their capital expenditures between 12 and 14 percentage points compared to the control group. This corresponds to a positive impact on capital expenditures of 0.70 to 0.80 standard deviations.

We estimate an average positive impact on cash flows of 1.13 million USD from account receivables and 0.98 million USD from inventories. Using the lower bound of the confidence intervals as a much more conservative estimate, the total impact on cash flow is at 0.19 million USD. This is a short term, one-off effect on cash flow as a result of the change in working capital. The reduction in account receivables might be related to the collection of existing receivables, potentially late ones, or due to the negotiation of new contracts with lower collection periods. Because we find a positive treatment effect on CAPX we estimate the corresponding cash out flow. We find an average cash flow impact of – 0.81 million USD, with a conservative estimate of 0.21 million USD.

2.4.4 Efficiency of Implemented Changes of Financial Policies (Financial Accounting Data)

Whether the implemented changes led to policies that are more efficient or not is not clear ex-ante. For instance, reducing inventories and collecting receivables earlier will increase free cash flows in the short run. However, there might be adverse effects in the long run if inventories become too low or if collection periods are too short (customers may be scared away because of products being out-of-stock or unattractive payment options, for instance).

To test whether firms have indeed moved towards more optimal policies as a response

to the treatment, we analyse whether treated firms become more efficient relative to the control group. Given that most firms are private, we do not observe their market values. Hence, we rely on accounting ratios such as return on assets (ROA) and return on invested capital (ROIC) to measure firm efficiency. We also analyse sales growth to test if there are any adverse effects on sales. One limitation of those accounting measures is that they are not forward-looking and only capture potential adverse effects that materializes in the short-run and we cannot exclude, for instance, that sales will be decreasing in a longer horizon.

Table 2.7 shows regression on firm performance. Panel A shows the treatment effect on ROA. We find a positive impact on firm performance between 0.21 and 0.22 using OLS and firm fixed effects, respectively. The effect on ROA is also statistically significant at the 5% level. The effect is equivalent to up to 0.85 standard deviations of ROA. In Panel B., columns (1)-(5) show results using a measure of return to capital invested (ROIC). The estimated coefficient is between 1.47 using OLS and 1.56 using firm fixed effects, which represents between 0.75 and 0.8 standard deviations of ROIC. This effect is statistically significant at 10% level. The point estimates of the most treatment effects are large, but not implausible, particularly given that the confidence intervals include more modest estimates as well.

Last, we analyse sales growth to test if there are any adverse effects of reducing inventories or collecting receivables more quickly. We do not find evidence of such an effect. The point estimates of the intervention on sales growth are actually positive though they are not statistically different from zero. Overall the results suggest that finance expertise of managers affect financial policies and that these policy changes can improve firm performance.

Table 2.7: Changes in Performance (Financial Data Regressions)

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

Panel A: Return on Assets (ROA)

	ROA				
	(1)	(2)	(3)	(4)	(5)
Treatment x Post	0.230**	0.205**	0.205**	0.212**	0.222**
	[0.108]	[0.100]	[0.099]	[0.100]	[0.102]
Treatment	-0.070				
	[0.089]				
Post	-0.198**	-0.211***	-0.211***		
	[0.075]	[0.071]	[0.072]		
Constant	0.241***				
	[0.081]				
Observations	519	519	519	519	519
R-squared	0.011	0.018	0.018	0.112	0.113
Firm FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes
Number of firm_id		76	76	76	76

Panel B: Return on invested capital (ROIC) and Sales Growth

	ROIC					Sales Growth				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x Post	1.471*	1.551*	1.551*	1.563*	1.545*	0.076	0.148	0.148	0.165	0.200
	[0.835]	[0.832]	[0.817]	[0.845]	[0.820]	[0.120]	[0.125]	[0.128]	[0.125]	[0.126]
Treatment	-0.497					-0.061				
	[0.411]					[0.048]				
Post	-0.960**	-1.014**	-1.014***			0.058	-0.031	-0.031		
	[0.398]	[0.388]	[0.368]			[0.080]	[0.085]	[0.089]		
Constant	0.829**					0.129***				
	[0.389]					[0.034]				
Observations	504	504	504	504	504	530	530	530	530	530
R-squared	0.014	0.018	0.018	0.042	0.043	0.005	0.003	0.003	0.192	0.194
Firm FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Control for size	No	No	No	No	Yes	No	No	No	No	Yes
Bootstrap s.e.	No	No	Yes	No	No	No	No	Yes	No	No
Clustered s.e.	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of firm_id		76	76	76	76		78	78	78	78

2.4.5 Threats to the Internal Validity and Robustness Tests

While the experimental setup hypothetically identifies the effect of the financial education programme on financial policies, there may be certain limitations that may affect the internal validity of the experiment in practice. In this section, we discuss some of these threats in more detail and provide additional tests on the internal validity.

2.4.5.1 Heterogeneity in Small Samples and Pre-trends

As described in Section 2.3, we have randomized the treatment status among firms that signed up for the programme and, by construction, there should be no systematic differences between treatment and control firm. However, in small samples, this is not necessarily true. There is the concern that, just by chance, there is heterogeneity between treatment and control groups that is driving our findings. Table 2.2, Panel A and B show that firms and managers of those firms are not systematically different. Almost all differences in mean and, especially, at the median are not statistically different across the two groups. However, given the small sample size, the power of those tests might be rather weak. For that reason, we also use the panel dimension of our data and test whether treatment and control firms are on common trends before the intervention. For the validity of our experiment, it is alright if the two groups were on different levels as long as they were not on different pre-trends (common trend assumption in difference-in-difference tests). We test this assumption non-parametrically, by plotting corresponding graphs for the main outcomes.

Figure 2.4 shows averages of those financial policies for firms in the treatment and control groups over the 2015-2018 period. The figures illustrate that, despite some differences in levels before the intervention, treatment and control group usually have parallel trends (especially in the year before the intervention between 2016 and 2017).

One exception are capital expenditures for which trends between the two groups appear to be different. However, in that specific case, the treatment group was actually on a positive trend before the intervention, while firms in the control group were decreasing their capital expenditures. Overall, the graphical analysis suggests that the parallel trends assumption is not violated, as treated and control groups follow parallel trends before the intervention across a majority of outcomes of interest.

2.4.5.2 Non-complying Firms

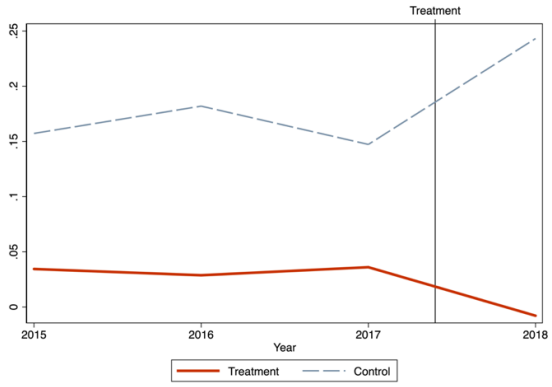
In our main analysis, we estimate the average effect of treatment on the treated (ATT) effect. While we have a very high compliance rate of about 91% (compared to about 53% in Bruhn et al. (2012), for instance), there is the concern that control firms that did not show up to the course may bias our results. Ex-ante, the direction of that bias is unclear though. For instance, it might be the case that only "good" firms do not show up for the course because they do not expect to profit from participating in the course; it might be also the case that firms that are in trouble do not show up for the course as they are too busy otherwise. Badly performing firms dropping out of the sample would indeed be in line with our results on ROA but it would be more difficult to tell a consistent story why those remaining firms do also decrease their working capital. In practice, we do not believe that there are systematic reasons though for why firms who initially enrolled in the course did not show up. For instance, two CEOs who initially expressed their interest in participating in the course (and were allocated to the treatment group) never replied to our invitation to enrol in the course; one other CEO had an unexpected meeting abroad during the period of the first intervention.

However, we can also include those four treatment firms that did not attend the course and estimate the intention-to-treat (ITT) effect of our intervention.[59] Table B1 in the

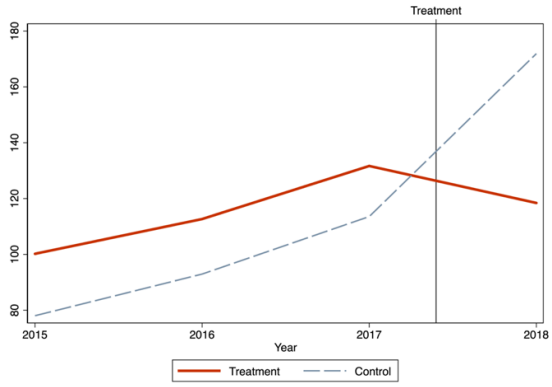
[59]Bruhn et al. (2018) estimate ITTs as their main specification.

Figure 2.4: Evolution of Selected Financial Outcomes

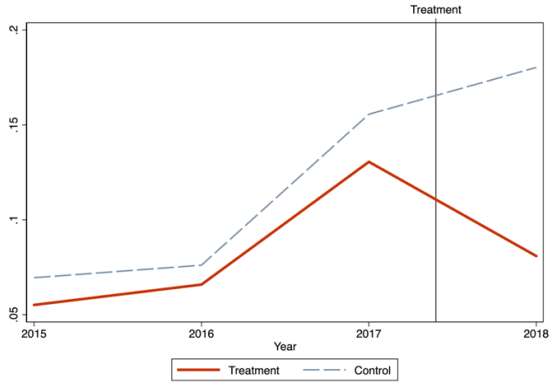
The graphs present mean financial outcomes over time for 85 firms included in the treatment and control samples. Financial outcomes are Working capital, Average Collection Period, Inventories, Capital Expenditure and Return on Assets (ROA). The vertical line denotes the date of the first intervention (treatment group). On the horizontal axis, each date represents the beginning of each year.



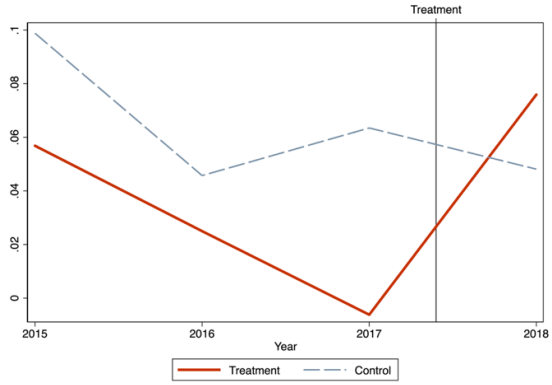
(a) Working Capital / (Lag) Assets



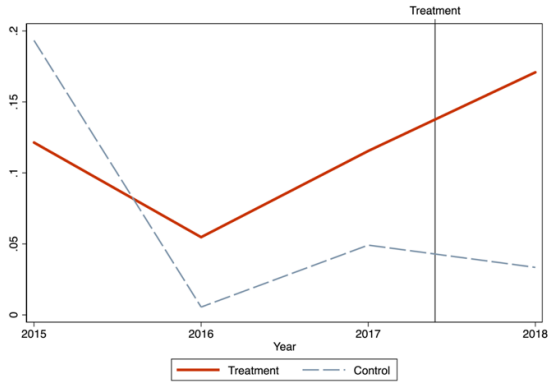
(b) Average Collection Period



(c) Inventory /Sales



(d) Capex/(Lag) Assets



(e) Return on Assets (ROA)

appendix shows the results for our main variables of interest. We do not find any strong evidence of certain types of firms systematically dropping out. The point estimates and standard errors remain basically unchanged.

2.4.5.3 Disclosure of Data

While all firms signed a data agreement at enrolment into the program, not all firms could share their data in the end. There is the concern that compliance to share financial data is systematically different for firms from the treatment and control group. In the case of ROA, for instance, it might be the case that well performing firms are more likely to share their financial data with us. If that was true for firms from both the treatment and the control group, the difference-in-difference estimates might be still unbiased though. It would be concerning though if badly performing firms from the treatment group were not to share their data but the same was not true for badly performing firms from the control group. In that case, sample selection could bias our findings.

We address this potential threat to the internal validity of our experiment in the following way. For a substantial subset of our data (i.e., firm-years), we also have access to external accounting data from a large accounting firm (KPMG) and that is not self-reported by the firms to us. This data should not suffer from the concern described above. We can use this external data to estimate a difference-in-difference effect for a subset of outcomes. Unfortunately, the granularity of the accounting data by the third party does not allow us to estimate the effect of the intervention on all different components of working capital. Table B2 in the appendix is showing the results for working capital and ROA. The point estimates have the same sign and are larger in absolute terms compared to our estimates when using all data, suggesting that -if anything- we

might be underestimating the magnitude of the effect in our baseline specifications.

2.4.5.4 Other Threats to the Internal Validity of the Experiment

In this section, we discuss other threats to the internal validity that are not explicitly testable (or we have not found a good way of doing so).

There is the concern that our experiment suffers from 'contamination'. For instance, we cannot fully rule out the possibility that treated managers shared their knowledge or course materials with managers of the control group, because Maputo is a relatively small city. However, it would actually work against us finding any results. We also tried to prevent the most likely contagion to occur by performing randomization at the business group level, as opposed to at the firm level. This implies that all managers from the same business group are part of the same cohort. Moreover, the treatment took place in a classroom setting, with an instructor and therefore it is unlikely that the control group would have access to the same treatment as the treatment group. The most plausible type of interaction between treatment and control groups could be the sharing of materials, for which close substitutes were already available, either online or in textbooks.

Another concern is that the managers of treated companies change their behavior, and update their expectations differently from the control firms, because of the intervention. For instance, managers might update their beliefs with respect to future firm performance and respond accordingly (see Chemla and Hennessy (2019)). In our setup this is less of a problem as both treatment and control managers expect to receive the same treatment.

2.4.5.5 Robustness Checks

We run a battery of additional robustness tests. First, some firms belong to the same business group. As a first robustness test, we exclude all non-core subsidiaries from our data. Table B3 shows the results for the main outcomes of interest. The results are unchanged (the point estimates are even slightly higher).

Then, we consider different time periods in our estimation of the treatment effect. Table B4 shows results for our main outcomes when we consider data after 2013, 2015, or 2016. While the point estimates slightly change depending on the time period, the qualitative results remain unchanged.

Because some firms experienced a CEO turnover during the period of the experiment, we have excluded them from the analysis. Table B5 shows the results which qualitatively remain unchanged. We have fewer observations though, and some coefficients are only significant at the 10-percent level.

We exclude firms that also operate in the financial industry (e.g., insurance companies). Table B6 shows the results. The main results remain unchanged.

Last, we use alternative definitions of financial ratios to measure working capital and accounting performance. In our main specifications we use the lagged value of book value of assets in the denominator. Table B7 displays the results when we scale the outcomes by contemporaneous book value of assets. The results are consistent with our baseline definition.

2.5 Interpretation and Policy Recommendations

While the experimental design helps to identify the treatment effect of the intervention, it remains unclear what exactly the channel is through which the executive education course on corporate finance affect financial policies. While answering this question is interesting in itself, it may also have important implications for policy.

The treatment, i.e., the participation in the executive education program, is basically a bundle of different simultaneous experiences: i) there is potential learning from the instructor, ii) there is potential learning from classmates, and iii) there might be aspects of the classroom experience, unrelated to the content of the course, that may affect outcomes (e.g., networking and generation of new deals between participants). It is difficult to identify the exact learning channel. However, we have several pieces of evidence that suggest that networking is not the main driver for our findings. While the results on ROA could be potentially in line with the hypothesis that treated firms interact with each other to generate new business, we do not find a significant impact on sales (see Panel B of Table 2.7). This is not too surprising as firms came from different segments and opportunities to establish business ties appear rather limited. Moreover, the documented changes in working capital are also not easy to reconcile with a network story. Last, we organized an event for the firms from the control group that took place around the dates of the first intervention. This event gave control firms the opportunity to get to know each other and to mingle. A remaining caveat is that the placebo-event was shorter than the 18-hours course for the treatment group and there might have been fewer opportunities to establish relationships. Taken together though, support for a networking explanation of the findings appears rather limited.

The importance of the classroom setting versus learning the content somewhere else, e.g., by self-studies or by enrolling into an online course, is also related to the question

what frictions prevented executives to obtain education on finance before. One potential reason is just unawareness of the importance of finance education for corporate efficiency. In that case, self-studies or the enrolment in online courses appear to be a good and cheap way of implementing financial education. Another reason could be the limited supply of such programs in Mozambique. Indeed, in Mozambique, there are no comparable executive education programs on finance (yet). Online courses or textbooks may be only very imperfect substitutes to a classroom education, that is led by a professor and making use of case discussions and active participation. To the best of our knowledge, the closest available programs are based in South Africa and expected costs (money and time-wise) are higher.

A second interesting question is whether participants learned something completely new or whether they were just reminded of the importance of some financial concepts. A hybrid version of these two extreme ends would be the case in which executives learned the foundations during (pre-experience) university degrees but only the professional experience combined with a more applied teaching method (e.g., case based) allowed them to apply the theoretical concepts in practice. We believe that a pure reminder (and versions of that such as the uptake of self-studies after the enrolment into our program) cannot explain the findings. Indeed, one advantage of our setup is that the control group knows that it will be treated as well and the enrolment to the program would remind both, treatment and control group.

From a policy point of view, it is not only important to know how to increase finance education among executives but also whether such an improvement of finance education is welfare improving. While we can certainly not answer this question with our setup, we believe it is still valuable to speculate about potential welfare implications. First, one may argue that large parts of the effect of the intervention go through accounts receivable. If accounts receivables of one firm go down, accounts payable of its

customers must go down as well and the overall impact on societal welfare is somehow unclear. One may also argue though, that some customers are from abroad including customers or firms of developed countries. In that case one could argue that the economy of Mozambique is likely to benefit. Moreover, we show that other policies such as inventories or capital expenditures are affected what may increase the productivity of firms. Finally, there might also be other policies that are more difficult to measure that benefit from the improved financial decisions.

Whether our findings can be generalized to other firms in or outside Mozambique is difficult to answer. However, we provide several tests that support the external validity of our findings. When we compare firms that applied to our program with other firms that are present in the KPMG reports but did not apply, we do not find them to be significantly different in terms of observable firm characteristics (Panel A of Table B8). We also compare the characteristics of participating executives with executives from firms in Mozambique that are present on the LinkedIn network. Panel B shows the results. When using the full LinkedIn sample we find no significant differences in tenure and MBA training. When we restrict the sample to include only firms with at least 25 employees or 100 followers, to better match our own sample in terms of firm size, we find no significant differences between the two samples except for gender. More interestingly, we also compare participants in our programme with the sample in Graham and Harvey (2001). Results are presented in Panel C. Again, we restrict the Graham and Harvey (2001) sample to firms of similar revenue to our sample. We also do not find any significant differences in tenure or education.

Overall our sample of firms and managers seems to be comparable to other firms and managers in Mozambique when it comes to observable characteristics. When it comes to the US, we also find managers' characteristics to be similar to the ones of firms of similar size in Mozambique.

2.6 Conclusion

This paper evaluates the impact of managers' financial expertise on firm financial policies and performance. A randomized controlled trial with top managers of 93 medium and large companies in Mozambique shows a positive effect on firm return on assets of an 18-hour executive education programme in finance. Our results suggest that deficiencies in managerial financial expertise at large firms can be an important constraint to firm growth.

Using survey data and firm financial information, we find that managers changed firm financial policies after the intervention. We find a significant and large treatment effect in working capital and average collection period. The effects on working capital management are large and significant: working capital decreases by 0.49 standard deviations for the treated firms when compared to the control group. This is likely to alleviate, at least in the short run, potential financial constraints. The effects on firm performance are economically relevant: ROA increases up to 0.85 standard deviations for treated group when compared to the control firms.

These results confirm that financial expertise of managers has a large impact on firm performance through the adoption of financial practices that promote value creation and alleviate financial constraints at the firm level. Moreover, our results suggest that relatively low-costs interventions such as an 18-hour executive education course on corporate finance and risk management can improve financial practices and decision-making and may ultimately affect economic development. In comparison, the experiment by Bloom et al. (2013) that was carried out in 28 plants operated by 17 firms ran about three years with a total consulting cost of USD 1.3 million, approximately USD 75,000 per treatment plant and USD 20,000 per control plant.

Appendix of Chapter 2

B1 Mozambique and other Sub-Saharan Economies

Survey statistics collected by the World Bank Enterprise Surveys (2018) suggest that the macroeconomic and investment environment in Mozambique is similar to that of other Sub-Saharan economies (henceforth SSA). In Mozambique, this survey covered 601 large, medium and small firms. In line with the purposes of our study, we have chosen some indicators that allow us to draw such analogy. For instance, regarding general firm characteristics, firms are, on average, 15 years old. In addition, both Mozambique and SSA share similarities in the ownership of firms. About 23% of firms have at least 10% of foreign ownership in Mozambique (16.6% in SSA) and 0.7% are government-owned (1.5% in SSA). Lastly, 44.5% of firms have their financial statements reviewed by external auditors in Mozambique, similarly to 48.6% of firms in SSA. Related to human capital, top managers have an average of 15 years of experience in the firms' working sector, both in Mozambique and in SSA. Interestingly, almost 16% of top managers in Mozambique and in SSA are female, a number that is approximated to the one in our sample.

Another important comparison is the credit market environment in Mozambique and SSA. Access to credit requires high collateral values (270% of the value of the loan in Mozambique and 220% in SSA). This leaves firms relying mostly on internal funds, 80% in Mozambique and 75% in SSA. Given these constraints in the credit market, 21% of firms in Mozambique use supplier/customer credit to finance working capital (25% in SSA).

B2 Participation in the Treatment

We also analyze the determinants of participation. The information obtained during the exploratory project is useful in this regard, as it allows analyzing enrolment rates (second stage) for different manager characteristics. Regarding gender, participation rates is higher among female respondents. Three out of six female participants we interviewed in 2015 ended up enrolling in 2017. The participation rate among male respondents is lower (39%). The participation rate is slightly higher among Mozambican respondents compared to other nationalities. When we analyze the roles, we also observed the enrolment rate is higher if we first met with the CFO (45%) then with general managers/CEO. The breakdown according to the maximum education attainment reveals that the participation rate is not monotonically related to education. Among participants with any higher education degree (excluding PhD), the enrolment rates vary between 37% to 47%. This pattern contrast with participants without any higher education degree (20%). Finally, we observe whether participation rates vary according to previous attendance of finance or accounting courses. We observe the course seems to be more attractive to the group of respondents that have no previous background in those fields (67%) compared to the ones who do (36%).

B3 Appendix Tables

Table B1: Intention-to-Treat (ITT) Estimates on Financial Policies

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available, as well as companies that were assigned to treatment but did not participated in the programme (Intention-to-Treat). The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**Avg. Collection Period**			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.177**	-0.215***	-0.201**	-74.507***	-63.648**	-62.233**	0.236**	0.212**	0.231**
	[0.083]	[0.079]	[0.078]	[24.895]	[24.390]	[26.682]	[0.105]	[0.098]	[0.099]
Treatment	-0.165**			15.282			-0.081		
	[0.074]			[25.655]			[0.088]		
Post	0.137**	0.100*		82.626***	78.472***		-0.198**	-0.211***	
	[0.059]	[0.056]		[22.528]	[22.831]		[0.075]	[0.071]	
Constant	0.185***			89.269***			0.241***		
	[0.052]			[15.456]			[0.081]		
Observations	427	427	427	222	222	222	538	538	538
R-squared	0.068	0.020	0.068	0.056	0.152	0.200	0.012	0.018	0.116
Firm FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		70	70		47	47		79	79

Table B2: External data only KPMG

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data from KPMG is available. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Post	-0.275***	-0.224***	-0.214***	0.416	0.502*	0.552*
	[0.094]	[0.081]	[0.076]	[0.253]	[0.289]	[0.320]
Treatment	-0.193**			-0.288		
	[0.081]			[0.254]		
Post	0.137*	0.083		-0.377	-0.417	
	[0.077]	[0.069]		[0.227]	[0.267]	
Constant	0.193***			0.452*		
	[0.059]			[0.246]		
Observations	321	321	321	315	315	315
R-squared	0.125	0.031	0.111	0.013	0.008	0.063
Firm FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		49	49		48	48

Table B3: Largest Segment Only

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. For each business group, only the largest segment is considered. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**Avg. Collection Period**			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.203*	-0.245**	-0.219**	-82.543***	-68.714**	-68.022**	0.235**	0.214**	0.229**
	[0.106]	[0.102]	[0.104]	[27.141]	[25.994]	[30.253]	[0.116]	[0.104]	[0.111]
Treatment	-0.186**			18.014			-0.014		
	[0.092]			[26.983]			[0.081]		
Post	0.159**	0.114*		76.335***	71.710***		-0.165***	-0.175***	
	[0.070]	[0.066]		[24.029]	[24.336]		[0.058]	[0.054]	
Constant	0.205***			89.805***			0.199***		
	[0.062]			[16.639]			[0.063]		
Observations	312	312	312	183	183	183	387	387	387
R-squared	0.074	0.022	0.074	0.057	0.144	0.166	0.011	0.016	0.135
Firm FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		51	51		39	39		55	55

Table B4: Different time periods

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The sample periods considered are from 2013, 2015 and 2016 until 2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**Avg. Collection Period**			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.173*	-0.185**	-0.213**	-62.596**	-70.517***	-62.199***	0.228***	0.133*	0.131**
	[0.090]	[0.082]	[0.085]	[27.558]	[25.427]	[22.017]	[0.084]	[0.077]	[0.061]
Observations	277	169	112	212	132	88	321	198	133
R-squared	0.051	0.093	0.161	0.198	0.220	0.298	0.151	0.035	0.076
Number of firm_id	66	63	59	45	45	45	74	71	71
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bootstrap s.e.	No	No	No	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years	≥2013	≥2015	≥2016	≥2013	≥2015	≥2016	≥2013	≥2015	≥2016

Table B5: Excluding CEO turnovers

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The analysis excludes all business groups for which the top manager has been replaced at any point during the research project. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**Avg. Collection Period**			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.121	-0.176*	-0.163*	-82.805***	-69.595***	-67.495**	0.252**	0.222**	0.220**
	[0.097]	[0.092]	[0.092]	[25.736]	[24.922]	[27.640]	[0.112]	[0.100]	[0.098]
Treatment	-0.135			30.626			-0.047		
	[0.094]			[30.004]			[0.078]		
Post	0.135**	0.095		82.626***	78.472***		-0.161**	-0.175***	
	[0.062]	[0.058]		[22.577]	[22.874]		[0.071]	[0.063]	
Constant	0.185***			89.269***			0.192***		
	[0.056]			[15.490]			[0.072]		
Observations	346	346	346	197	197	197	458	458	458
R-squared	0.041	0.013	0.057	0.061	0.152	0.204	0.011	0.019	0.104
Firm FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		58	58		42	42		67	67

Table B6: Excluding Financial Companies

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. The analysis excludes companies in financial sector. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**Avg. Collection Period**			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment x Post	-0.161*	-0.219**	-0.204**	-76.430***	-71.285**	-65.590**	0.239**	0.217*	0.231**
	[0.094]	[0.091]	[0.090]	[26.990]	[26.987]	[29.475]	[0.116]	[0.109]	[0.110]
Treatment	-0.159**			16.831			-0.065		
	[0.076]			[27.847]			[0.096]		
Post	0.133**	0.099		88.863***	85.116***		-0.205**	-0.222***	
	[0.063]	[0.061]		[25.245]	[25.314]		[0.081]	[0.077]	
Constant	0.153***			89.163***			0.249***		
	[0.046]			[16.464]			[0.087]		
Observations	372	372	372	195	195	195	481	481	481
R-squared	0.062	0.019	0.073	0.063	0.165	0.221	0.010	0.018	0.119
Firm FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		63	63		41	41		71	71

Table B7: Outcomes scaled by contemporaneous total assets

The table displays the difference in difference estimator for firm financial performance. The sample includes treated and control firms that participated in the programme for which financial data is available. Working Capital and ROA are scaled by contemporaneous book value of total assets. The sample period is 2008-2018. *, **, *** Significance at 10, 5 and 1%, respectively.

	Working Capital			**ROA**		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Post	-0.070	-0.106*	-0.110*	0.198**	0.194**	0.198**
	[0.065]	[0.063]	[0.057]	[0.093]	[0.090]	[0.090]
Treatment	-0.181***			-0.061		
	[0.068]			[0.073]		
Post	0.052	0.024		-0.167***	-0.191***	
	[0.044]	[0.043]		[0.061]	[0.060]	
Constant	0.191***			0.200***		
	[0.045]			[0.065]		
Observations	466	466	466	607	607	607
R-squared	0.082	0.008	0.031	0.010	0.016	0.090
Firm FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes
Control for size	No	No	Yes	No	No	Yes
Bootstrap s.e.	No	No	No	No	No	No
Clustered s.e.	Yes	Yes	Yes	Yes	Yes	Yes
Number of firm_id		71	71		81	81

Table B8: External Validity

Panel A displays summary statistics for the main financial variables of 93 firms participating in the programme (Treated/Control Sample), and for firms that did not enrol in the programme. Financial data is obtained from KPMG "Top-100 Companies in Mozambique" report, and hand collected. Panel B presents a comparison between top managers in our sample and a representative sample obtained through LinkedIn. The LinkedIn sample was obtained through manual extraction on LinkedIn using the following filters: location (Mozambique) and title (CEO/General Manager/CEO/Financial Director). The search occurred on the 17th of July 2019 and we obtained 790 entries (current CEO/CFO). The first two columns exhibit descriptive statistics on the top managers of treatment and control companies (pooled) in our sample. The next two columns correspond to the aggregate LinkedIn sample. In the following four columns, we condition the analysis to executives of companies with more than 25 (registered) employees or at least 100 followers. The significant reduction in the number of observations is due to many missing data on employees and followers. In Panel C, we present the statistics computed on Graham and Harvey (2001) survey data. The category "Masters or higher" contains the "MBA" category. In panels B and C, we present t-test statistics for the mean difference between our sample and LinkedIn or Graham and Harvey (2001) samples, respectively. *, **, *** Significance at 10, 5 and 1%, respectively.

Panel A: Enrolment in the Programme: Firms

	Enrolled Companies				**Non-enrolled Companies**				**Mean**		**Median**	
	Obs.	**Mean**	**Median**	**St.Dev.**	**Obs.**	**Mean**	**Median**	**St.Dev.**	**Diff.**	**p-value**	**Diff.**	**p-value**
Total Assets (m USD)	78	93.54	6.61	285.76	173	80.32	5.39	258.29	13.22	0.72	1.22	0.65
Sales (m USD)	78	44.95	6.24	113.13	172	28.92	3.74	84.87	16.03	0.22	2.50	0.22
Sales Growth	74	-0.23	-0.30	0.58	135	-0.14	-0.25	0.61	-0.09	0.28	-0.05	0.34
# Employees	72	307.81	99.5	605.69	172	440.37	89.5	1,327.05	-133.00	0.42	10.00	0.67
Leverage	59	0.22	0.11	0.30	153	0.21	0.04	0.32	0.01	0.95	0.07	0.36
Capex / Assets	61	0.04	0.01	0.26	66	0.73	0.02	5.38	-0.69	0.32	-0.01	0.18
Return on Assets (ROA)	76	0.08	0.06	0.24	173	0.03	0.04	0.28	0.05	0.21	0.02	0.65
Return on Inv. Cap. (ROIC)	75	0.39	0.15	1.91	170	0.45	0.19	1.92	-0.06	0.83	-0.04	0.61
Working Capital/ (Lag) Assets	64	0.10	0.12	0.35	73	0.02	0.01	0.36	0.08	0.18	0.11	0.11
Working Capital / Sales	64	0.12	0.09	0.71	73	-0.07	0.01	0.88	0.19	0.19	0.08	0.20

Panel B: Participating Managers' Characteristics vs LinkedIn

	Our Sample		LinkedIn						Differences					
			Full Sample		Employees ≥25		Followers ≥100		Full Sample		Employees ≥25		Followers ≥100	
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Obs.	Mean	Diff.	p-value	Diff.	p-value	Diff.	p-value
Male	67	78%	790	92%	50	96%	67	94%	-0.14	0.00***	-0.18	0.01***	-0.16	0.01***
Tenure (years)	66	7.45	761	6.98	48	7.67	66	7.39	0.47	0.59	-0.22	0.86	0.06	0.96
Masters or higher	63	49%	407	37%	29	52%	41	49%	0.12	0.06*	-0.03	0.82	0.00	0.97
MBA	63	21%	407	15%	29	28%	41	20%	0.06	0.25	-0.07	0.47	0.01	0.89

Panel C: Participating Managers' Characteristics vs Graham and Harvey (2001)

	Our Sample		Graham and Harvey (2001)				Differences			
			(Full Sample)		(Sales≤25)		(Full Sample)		(Sales≤25)	
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Diff.	p-value	Diff.	p-value
Male	67	78%	-	-	-	-	-	-	-	-
Tenure (years)	66	7.45	366	6.68	92	7.59	0.76	0.20	-0.14	0.87
Masters or higher	63	49%	354	60%	91	52%	-0.11	0.11	-0.02	0.77
MBA	63	21%	354	38%	91	27%	-0.18	0.01***	-0.07	0.34

Table B9: Variables description

This table presents a description of each variable as well as its sources.

Variables	Description	Source		
		Hand Coll.	KPMG	Survey
Total Assets (m USD)	Total Assets (book value) (million USD).	YES	YES	
Sales (m USD)	Revenue (million USD).	YES	YES	
Sales Growth	Percentage change in revenue relative to previous year.	YES	YES	
# Employees	Number of employees.	YES	YES	
Cash / (Lag) Assets	Cash over one-year lagged total assets.	YES	YES	
Leverage	Long-term total liabilities over one-year lagged total assets.	YES	YES	
Capex / (Lag) Assets	Capital expenditure over one-year lagged total assets. Capital expenditure is computed as property, plant and equipment minus one-year lagged property, plant and equipment plus depreciation and amortization.	YES	YES	
Return on (Lag) Assets	Operating income over total assets. Operating income is defined as revenues minus operating costs.	YES	YES	
Return on Inv. Cap.	Operating income over total assets minus current liabilities. Operating income is defined as revenues minus operating costs.	YES	YES	
Working Capital / (Lag) Assets	Working Capital over one-year lagged total assets. Working capital is defined as current assets minus current liabilities.	YES	YES	
Working Capital / Sales	Working Capital over sales.	YES	YES	
Inventories / Sales	Inventories over sales.	YES		
A/R / Sales	Accounts receivable over sales.	YES		
A/P / Sales	Accounts payable over sales.	YES		
Avg. Collection Period	Accounts receivable over sales times 365 (days).	YES		
Male	Male.			YES
Age (years)	Age in years.			YES
Tenure (years)	Current tenure.			YES
Mozambican	Mozambican nationality.			YES
CEO	CEO/General Manager/Managing Partner			YES
CFO	CFO/Financial Director/Head of Financial Department.			YES
Masters of higher	Highest educational attainment higher or equal than masters. Includes the following categories: masters, post-graduation, MBA and PhD.			YES
MBA	Highest educational attainment in MBA.			YES
Acc. or Finance Background	Manager has attained accounting and finance courses at any education level.			YES
Treatment	Treatment equals 1 to all companies or business groups that were assigned to treatment. In the main specifications, treatment is defined following an ATT approach, i.e. we do not classify as treatment those companies that did not adhere to the randomized allocation. In the robustness section, we present another definition of treatment where those companies are included in the treatment group (ITT).			
Post	Post equals 1 in 2017 (year-end).			

B4 Webpage, Brochure, and Course Description

Exhibit A: The Webpage at Imperial College London Business School

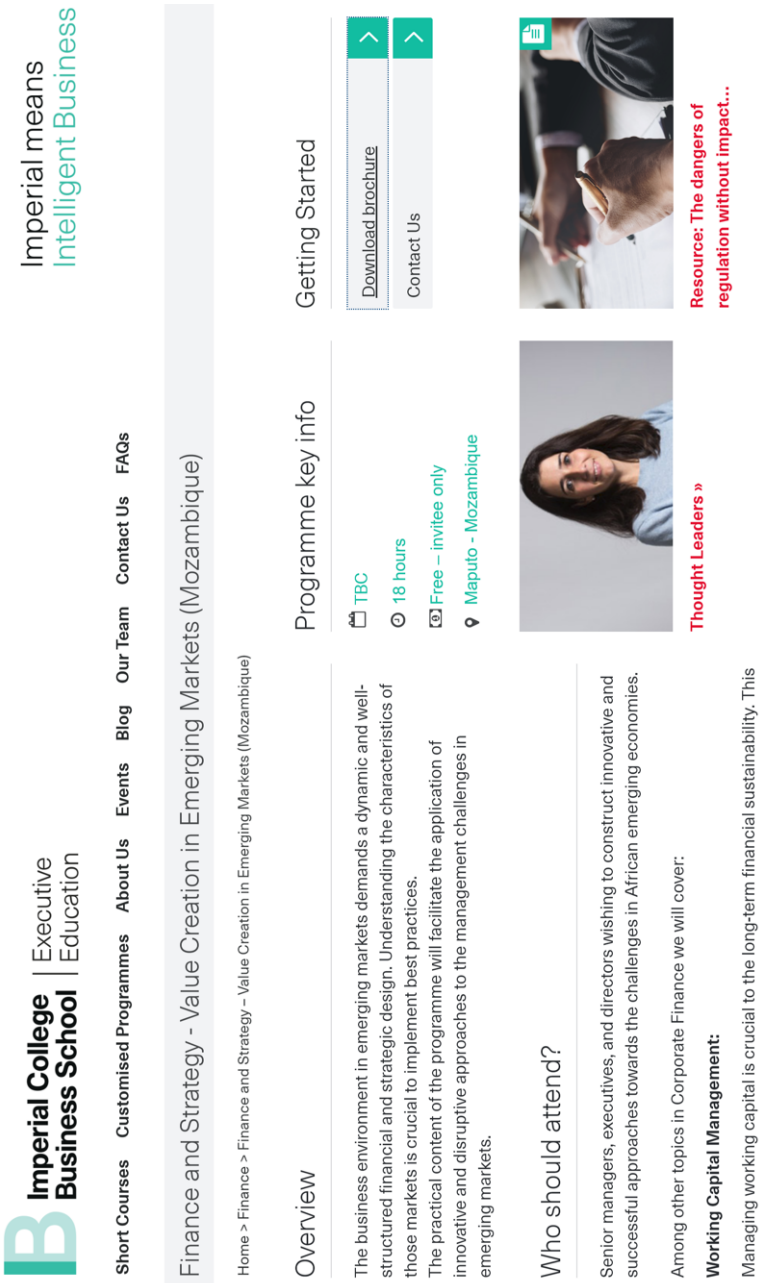


Exhibit B: The Brochure

Overview

The business environment in emerging markets demands a dynamic and well-structured financial and strategic design. Understanding the characteristics of those markets is crucial to implement best practices.

The practical content of the programme will facilitate the application of innovative and disruptive approaches to the management challenges in emerging markets.

Descrição

O ambiente empresarial em mercados emergentes requer um desenho financeiro e estratégico dinâmico e estruturado.

Compreender as características desses mercados é, portanto, essencial na implementação de boas práticas.

O conteúdo prático do programa irá facilitar a aplicação de abordagens inovadoras e disruptivas face aos desafios que a gestão em mercados emergentes coloca.

Who should attend?

Senior managers, executives, and directors wishing to construct innovative and successful approaches towards the challenges in African emerging economies.

A quem se destina?

Gestores seniores, executivos e directores que desejem construir abordagens inovadoras e com sucesso face aos desafios que se colocam em mercados emergentes africanos.

All participants will be awarded a Certificate of Attendance from Imperial College Business School Executive Education by the end of the course.
A todos os participantes será concedido um Certificado de Presença emitido pelo Imperial College Business School Executive Education.

Among other topics in Corporate Finance we will cover:

Working Capital Management

Managing working capital is crucial to the long-term financial sustainability. This section discusses how prioritizing working capital allows companies to make strategic investments, which in turn drive operational efficiencies.

Capital Structure

Financial leverage constitutes an important part of a corporation's day-to-day operations. Given the ever-changing market conditions, it is important to understand how a firm can achieve its optimal leverage ratio, and the benefits, costs and risks associated with it. Participants will gain hands-on practice with rigorous methods to account for leverage in a firm's capital structure.

Risk Management

Insight on the use of value enhancing and risk reducing strategies constitute a capital set of skills in the current business environment. This section aims to guide you towards the development of financial foresight, allowing for the prediction of new financial and corporate risks.

Valuation

Rigorous understanding of valuation allows you to maximise the potential of a company, drive investment decisions and lead the restructuring, M&A and financing challenges that your organization faces.

Entre outros tópicos em Finanças da Empresa, nós iremos abordar:

Gestão de Fundo de Maneio

Gerir o fundo de maneio de uma empresa é crucial para a sustentabilidade de longo prazo. Esta secção visa entender a importância do fundo de maneio na tomada de decisões estratégicas, com vista a gerar mais-valias operacionais.

Estrutura de Capital

A alavancagem financeira integra o dia-a-dia operacional de uma empresa. Considerando o dinamismo dos mercados, é importante entender como uma empresa pode atingir o seu nível óptimo de dívida, assim como os benefícios, custos e riscos associados. Os participantes irão ganhar uma visão prática sobre métodos rigorosos na análise de estrutura de capital.

Gestão de Risco

O conhecimento de estratégias de gestão de risco constitui uma valência de extrema importância nas atuais condições de mercado. Esta secção tem como objectivo o desenvolvimento de técnicas de previsão financeira, nomeadamente na antecipação de novos riscos financeiros e empresariais.

Avaliação de Projectos

O conhecimento rigoroso de métodos de avaliação permite a maximização do potencial de uma empresa, e auxilia na tomada de decisões de investimento, restruturação, M&A e financiamento que as empresas enfrentam.

Exhibit C: The Schedule of the Course

Day 1 – Morning	**Day 1 – Afternoon**	**Day 2 – Morning**	**Day 2 – Afternoon**
The basics: time value of money; investment decision rules **Capital budgeting and Valuation**	**Working capital management**	**Capital Structure (Debt vs. Equity decisions)**	**Risk Management (Insurance and Hedging decisions)**
The case of **New Earth Mining** (Capital budgeting and valuation in emerging markets)	The case of **Fibria Celulose SA and Procter and Gamble** (Working capital in emerging markets)	The case of **UST** (Leverage recapitalization)	The case of **Mozal** (Risk Management)

Chapter 3

Central Government Performance and Local Election Outcomes: A Randomized Experiment

This chapter was written in co-authorship with Bruno Carvalho, Cláudia Custódio, Susana Peralta and Benny Geys. This work was funded by Fundação para a Ciência e Tecnologia (UID/ECO/00124/2013 and Social Sciences Data Lab, Project 22209), POR Lisboa (LISBOA-0-0145-FEDER-007722 and Social Sciences Data Lab, Project 22209) and POR Norte (Social Sciences Data Lab, Project 22209). We are deeply thankful for the comments of Miguel Portela, Leif Helland, Jon Fiva and Pedro Freitas. Susana Peralta wrote this paper while visiting *Banco de Portugal*, whose hospitality is gratefully acknowledged. We also thank *Nova School of Business and Economics* and *Instituto de Contabilidade e Administração de Lisboa* and all its professors and students for the interest and availability to participate in the experiment.

3.1 Introduction

The recent debate about how the so-called *fake news* may have influenced the 2016 US presidential election, the 2016 Brexit referendum or the 2018 Brazilian election has sparked renewed interest in the role of information on electoral outcomes (Allcott and Gentzkow, 2017; King et al., 2017). Election periods are awash with official campaign coverage through outdoor and social media advertising, interviews and debates where political agents either self-promote or attack other candidates. It is of crucial importance to understand how people react to this information when shaping their electoral behavior.

Much of the information spread out around election periods is not related with the government layer for which elections are being held. In sub-national elections, for instance, it is common that politicians aligned with upper-level governments try to capitalize on their policies, while the opposition tries to point out their flaws.

In this paper we ask whether the perception about central government performance affects individual voting behavior at municipal elections. To address this research question, we conducted a randomized controlled trial where we exogenously induce changes in perception about central government performance and then evaluate how these changes impact local voting behavior.

Our experiment exposed 1800 students of two business schools in Lisbon to an information bundle about the central government in charge during the 2017 Portuguese local election, held on October 1st. In the two weeks prior to the election, we ran a baseline survey to collected respondents' socio-demographic and political characteristics, and then we conducted the randomized intervention. In the week following the election, a follow-up survey was implemented to gather information about self-reported realized voting outcomes.

The treatment consisted of exposing participants to positive, neutral or negative-tone news articles about central government activity. Our design allows us to explore how the tone of the information provided affects perception updating. News articles were selected from national media outlets and covered policy areas relevant to central government action, ranging from macroeconomic topics (public finance, pensions and youth employment) to some specific areas (national health service, road safety and education). For each policy area, we selected two news articles from the same source and within a negligible time gap: one conveying a negative and another conveying a positive message about central government action. The articles were then combined to generate 12 different treatment versions: 6 versions conveying negative information and another 6 conveying positive information. The control group received neutral information regarding government activity, namely about a Portuguese, non-endangered, dog breed.

Some important contributions of our work derive from our experimental design. First, the information provided is truthful and taken from credible media outlets. In that way, we guarantee that asymmetric responses to the information are not driven by differences in the credibility that respondents assign to each of the news. This allows us to avoid a caveat in James (2011), where the negative information provided is not factual. As the author points out, this negative information may contradict prior beliefs and thus affect the respondents' reaction to the treatment. Second, our 12 different treatment versions cover a variety of topics, which allows us to measure the reaction to the tone of the message, without being tied to the particularities of a specific policy area. Although Olsen (2015) explores the reaction to equivalent satisfaction and dissatisfaction measures about the same factual information, the experiment relies on information about the Danish hospital services, which may have different salience depending on the respondents. Lastly, because for each positive-tone information we have a comparable negative survey covering the same topics, we can ensure the reaction to the treatment

does not stem from the negative information being more interesting or relevant than the positive one, for instance. The treatment in Soroka and McAdams (2015), who conduct a psycho-physiological experiment and show that viewers react more to negative-tone TV news, encompasses a variety of subjects, however negative and positive information is not directly comparable.

The goal of our informational treatment is to induce exogenous variation in the perceived performance of the central government activity. We start by showing how participants' perception regarding the specific policy areas covered is affected by the treatment. Both positive and negative information have the expected impacts: negative treatment induces a downward revision while positive treatment translates into an upward assessment of government performance. This result is robust to several specifications and to the inclusion of controls. Then, we evaluate how respondents aggregate this updated policy-specific perception into a general performance perception about the central government. In this case, the impact of positive information declines in size and in most specifications loses significance. Conversely, the negative treatment has a robust and sizable effect on the general perception of central government performance. We thus find evidence that negative information is more salient than positive information in our study.

This finding, known as *negativity bias*, reinforces the conclusions of several previous studies both in political and non-political contexts. Lau (1982, 1985), for instance, discusses why and how different dimensions of negativity affect political behavior, using survey experiments. Using Danish data, Nannestad and Paldam (1997) show that economic voting is more pronounced when the economic activity is slowing down. James and John (2007) study the impact of performance information on electoral support for the incumbent, finding that incumbents with low performance are punished in the polls. More recently, Lockwood and Rockey (2015) frame negativity bias in terms of loss

aversion in a probabilistic voting model and shows, using data on elections in the US, that loss aversion affects both electoral competition and election outcomes. Based on survey data from the UK, Tilley et al. (2018) show that voters punish incumbent governments in elections as a reaction to the worsening of personal financial circumstances, particularly if that reduction derives from government actions. Regarding non-political settings, Baumeister et al. (2001) discuss the reasons why negative information or experiences have stronger effects on a broad range of psychological phenomena. In turn, James and Moseley (2014) perform a field experiment in two British municipalities and shows that information about low absolute performance of waste recycling services lowers citizen satisfaction with no corresponding effect for positive information.

Taking advantage of the comprehensive information we gathered about baseline respondents' characteristics, we investigate whether the information provided triggers a different response according to levels of awareness. We use two proxies for awareness. One is the self-reported interest degree in each policy area covered in the news articles. A second measure relies on parents' occupation as a proxy for access to information. For example, participants whose parents are doctors are expected to have higher *ex-ante* exposure to information about the National Health System. Using these two measures, we find evidence that the *ex-ante* stock of information plays a role: the response to information is lower among participants with higher levels of awareness. This fact illustrates the common view that *fake news* may spread out more easily among less literate sectors of population.

Regarding our second research question, we assess whether the exogenously-induced change in perception about government performance impacts on local election behavior. The connection between the two may occur for several reasons. First, voters can see the party system as a selection mechanism for local candidates (Geys and Vermeir, 2014). Hence, whenever the party in charge of the government is performing well, local

candidates aligned with that party can be perceived as more capable. Second, local elections can be seen as second order elections. In this case, voting behavior in such elections can be a channel to send a message of approval or punishment to the central government (Marien et al., 2015). Lastly, voters may expect to draw benefits from having local representatives with closer ties to central government, such as an increase in public transfers to their municipalities (Fiva and Halse, 2016).

We focus on turnout and voting decisions as local election outcomes.[60] If a connection between central politics and local voting exists, the probability of a local candidate aligned with the parties ruling at the central level getting a vote would increase with the positive treatment and decrease with the negative one. Our findings suggest that there is no average treatment effect on neither turnout nor on the local candidate chosen. The non-significant treatment effect can be interpreted in at least two ways. On the one hand, local election behavior may not be driven by the perception about central government performance. In this case, it is less likely that local elections work as second order elections, where voting would be essentially driven by non-local factors. On the other hand, it may arise due to a treatment dilution problem. As a robustness test, we apply an instrumental variable approach to deal with this concern, as suggested in Angrist (2006). Specifically, we instrument the post-treatment general government performance assessment with the treatment assignment. The fitted values of this regression are then used to assess the impact of the treatment on voting outcomes. Still, the evidence of the impact is weak. Thus, our results suggest that central government performance is not a key driver of voting on local elections.

Although we do not find a significant treatment effect on local election outcomes, central

[60]There is a substantial literature examining the determinants of voter turnout, see Cancela and Geys (2016) for a meta-analysis and discussion. Some of these studies, like Kahn and Kenney (1999); Moeller et al. (2014) or Baekgaard et al. (2014), focus on the impact that information provision has on turnout.

government performance may be relevant for the subsets of voters for whom voting preferences are less rigid. In our sample, first time voters and voters who were still undecided about the candidate on whom to vote in the week before the election can be seen as two of these subsets. We do not find significant differences on first time voters' behavior. However, among the subset of undecided voters, we show that the negative treatment increases the likelihood of voting for parties not associated with the central government in power (mainly opposition parties). This change is mostly driven by a reduction in the likelihood of casting a blank vote. In this light, our results show that information affecting the perception about central government performance may be relevant when it comes to undecided or potentially swing voters.

By evaluating how performance-related information affects election outcomes, our work provides an important contribution to the literature on economic voting. In a seminal paper, Kramer (1971) uses aggregate data to evaluate whether national economic conditions and the perceived performance of the incumbent party affect voting behavior for the US House of Representatives. More recently, James (2011) shows that published information influences performance perception and satisfaction, particularly when partisan effects are present. In a more aggregate perspective, Taniguchi (2016) evaluates whether economic indicators can act as a determinant of upper house elections in Japan, while Kappe (2018) uses UK data to evaluate reference-point-dependent behavior in economic voting. From a theoretical standpoint, Ashworth et al. (2018) show that voter rationality can be reconciled with the effects of exogenous shocks (outside of the control of politicians, like natural disasters) affecting the probability of reelection of an incumbent. Economic voting in multilevel governance is discussed in Anderson (2006), who shows that economic voting is weaker in countries with stronger multilevel governance, particularly when responsibilities are shared by different government levels. In Portugal, Martins and Veiga (2013) show that the countries' economic performance

affects the success of candidates in local municipal elections, particularly if they are from the same party of the incumbent central government. While many of these studies report positive correlations between economic performance and the likelihood of incumbents to be reelected, the quasi-experimental evidence is limited.

Our work also contributes to the literature on second order elections. Clark and Rohrschneider (2009) and Schakel and Jeffery (2013) evaluate the relationship between voting behavior in European elections and national development and Marien et al. (2015) studies the relationship between voting in local and federal elections in Belgium. Additionally, our work relates to the literature on media coverage impact and bias. Soroka (2006), DellaVigna and Kaplan (2007), Garz (2014), Eberl et al. (2017) and Haselmayer et al. (2017) do empirical assessments of media coverage (in some cases, specifically negative news) and their impacts on election outcomes. It is also related with studies that evaluate messages conveyed during the campaign period, either about candidates' skills or ideology. The first work about negative campaigning effects was developed by Ansolabehere et al. (1994), where the authors show that negative campaigning reduces voter turnout through an experiment in California. More recently, in a field experiment around the 2004 US presidential election, Arceneaux and Nickerson (2010) show that in-person delivered messages can influence voting preferences. Kendall et al. (2015) performed a large scale experiment in an Italian municipality, during the 2011 local elections, concluding that information treatments (particularly about candidate skills) impact actual vote and individual vote declarations. Finally, in a relatively new branch of literature that explores citizen heterogeneity and its influence on campaigning effectiveness, Galasso and Nannicini (2016) conducted a survey and a natural experiment in two municipal elections in Italy, finding that females react more to positive information, while males to negative information.

The remaining of the paper is organized as follows. We start with an overview of the

political institutional setting in Portugal, a description of the experimental design and estimation strategy in section 3.2. In section 3.3 we present the data. In sections 3.4 and 3.5 we present the main results. We conclude in the final section.

3.2 Experimental Design and Methodology

This section begins with a description of the institutional setting. Next, we describe the experimental design including its timeline, the structure of the implementation process and the randomization procedure. By the end of the section, we present the estimation strategy.[61]

3.2.1 Institutional Setting

The Portuguese political administration is organized in three levels. The first level comprises the central government. The second level corresponds to municipalities. There are 308 municipalities in Portugal. Municipalities are then divided into a third administrative division comprised by civil parishes. Municipalities and civil parishes represent the local government and together are responsible for improving the well-being of inhabitants through the promotion of social and economic development, territorial planning and local public goods provision.

Local government representatives are elected simultaneously in a nationwide local election taking place every four years. The municipal government is composed of an executive branch – the Town Council – and a legislative one – the Municipal Assembly. Both bodies are directly elected and seats are allocated to parties proportionally following the d'Hondt method according to the number of votes. The first ranked candidate of the

[61] This experiment was registered at the American Economic Association, the information submitted can be found at https://www.socialscienceregistry.org/trials/2539.

most voted party for the Town Council is appointed as Mayor. The Town Council, and the Mayor in specific, is the most prominent administrative body at local government level. Central government elections are held every four years, differing from the local election by a two-year period.

The Portuguese political landscape is dominated by five main parties that run for both central and local government elections.[62] From left to right, those are *Bloco de Esquerda* (BE), *Coligação Democrática Unitária* (CDU), *Partido Socialista* (PS), *Partido Social-Democrata* (PSD) and *Centro Democrático e Social* (CDS). The low degree of fragmentation of the political scene may potentially create room for connection between central and local government outcomes through the party system.

Our experiment took place around the 2017 local election. In this election, PS emerged as the main winner by appointing 159 out of the 308 mayors, while the main opposition party (PSD) has appointed 98 mayors. The previous central government election had occurred two years ago, in 2015. In the 2015 election, the incumbent centre-right coalition (PSD + CDS) obtained the highest vote share but failed to secure a majority in the parliament. In turn, PS was able to obtain a majority supported by left-wing parties (BE + CDU) and was appointed to form a government.[63] By the time of our experiment, participants are expected to have had enough time to build up a belief about performance of the government in charge.

[62]Together, either running alone or in coalition, these parties gathered 86% of all votes for the Town Council in 2017. The remaining candidates include other parties, independent candidates and formally organized groups of citizens.

[63]The centre-right coalition (PSD + CDS) obtained 38,5% of the votes, while PS, BE and CDU obtained 32,32%, 10,19% and 8,25%, respectively.

3.2.2 Experimental Design

We aim to evaluate whether central government performance impacts the voting decision in local government elections. Addressing this question poses an identification challenge. While some people might consider current government performance as good or acceptable, others might actually regard it as bad or unacceptable. Therefore, there might be a large heterogeneity regarding perceived performance. A potential problem of this is that perceived performance may be determined by factors that also affect local election behavior. One example may be political alignment. In order to identify a causal impact of central government performance on local election outcomes, one needs exogenous variation in perceived performance.

In this experiment we propose to induce exogenous variation in perceived performance by exposing participants to factual information about current government action. We rely on information as a driving mechanism of perceived performance. This assumption is not straightforward though and it leads us to pursue our goal in two stages:

1. Is information a driving mechanism of perceived perception?
2. Does (exogenously driven) perceived performance about central government impacts local election behavior?

We implemented a randomized controlled trial at the time of the 2017 local election. The treatment consisted of exposing participants to positive, neutral or negative-tone information about central government performance. We denote these groups as positive treatment, control and negative treatment, respectively. Each respondent was presented with two news articles about different central government policy areas, adapted from mainstream national media outlets.[64]

[64]News were edited by eliminating less relevant sentences with the purpose of focusing readers' attention on the most important parts. All information has been presented in Portuguese.

The articles were selected following four predefined guidelines. Firstly, they should be as recent as possible. Secondly, central government policy areas with varying degrees of media coverage should be selected. We selected articles on the following policy areas: public finance (public debt and investment), social security (pensions), national health system, national education system, youth employment and road safety. Then, for each policy area, positive- and negative-tone news should be available from the same source and within a negligible time gap. For instance, if a positive treatment consists of two positive news articles about public finance and pensions, then there is a corresponding version with two negative-tone articles on the same areas, from the same media outlets. Finally, to the extent possible we aimed at diversifying media outlets. We end up selecting articles from two newspapers and one radio station.

As an example, on the 6th of September 2017, a national radio station (TSF) reported that the Ministry of Education planned to hire 1 500 auxiliary teaching staff over the next school year. Two days later, the same source reported that there were schools delaying the beginning of the school year due to staff shortage. Another example is the National Health System, where the positive news is about a higher budget allocation to the Ministry of Health, while the negative reports an increase in the outstanding debt of the National Health System.

We created 13 information bundles – six positive, six negative, and one neutral information bundle – according to the following structure. The first three information bundles contain a first article in public finance and a second article in either youth employment, road safety, or pensions. The remaining three bundles include the article on the national health system together with either youth employment, road safety, or national education system. It is important to highlight that public finance was attracting substantial media coverage by the time of the experiment, with the government claiming a substantial improvement in the country's financial position. In contrast,

policies targeting the national health system received considerably lower media attention in the months before the election. The remaining policy areas were chosen to cover both macroeconomic outcomes (youth employment and pensions) and microeconomic policies (road safety and education) in order to minimize the likelihood of our results being driven by specific policy areas. The control group was presented with two news articles about a non-endangered Portuguese dog breed, the *Serra da Estrela*, designed to have a similar extension and format as the treatment ones. A detailed description of all information bundles is provided in panel B of table C1.

The format chosen for the information bundles achieves several goals. Firstly, combining two news articles in each information bundle increases the probability of conveying new information. Even if the participant is aware of information contained in one of the two articles, it is less likely that he or she has had previous access to both. Secondly, relying on factual and credible information allows us to avoid the caveat in James (2011), who does not find evidence of negative bias in a set of two lab experiments. As the author points out, the negative information made available for respondents was not real and was instead created to mimic the positive information. Thus, the absence of negativity bias may be associated with the non-credible information provided, contradicting the prior perception of the citizens. Finally, by creating a large set of information bundles, we can disentangle the impact of giving information about a given policy area from the actual tone of the message, besides reducing the likelihood of contagion across different treatment groups.[65]

The experimental design encompassed three survey rounds. A detailed implementation calendar is shown in figure 3.1. A baseline survey was implemented two weeks before the election and aimed at collecting baseline data such as socio-demographic context,

[65]This is an important concern given that surveys were administered in a classroom setting, where treatment and control subjects responded to the survey side-by-side.

political preferences, awareness and motivation. Respondents were also asked about their intended voting behavior in the local election taking place within two weeks. The second survey round (treatment survey) took place in the week before the election. In this round we carried out the intervention by randomly exposing participants to one of the 13 information bundles. Each information bundle appeared in the middle of the treatment survey and included the headline (in bold bigger font) and the news article. The first article included a graph created by the researchers based on real statistics to support the information conveyed in the text. The graphs were built to be visually similar across the positive and negative-tone news. The second news article was accompanied by an illustrative picture common to the positive and the negative-tone article. An example of a negative and the corresponding positive information bundle is provided in figures C1 and C2.[66] The follow-up survey was implemented in the week following the election with the purpose of collecting self-reported voting behavior. Panel A of table C1 summarizes the type of information collected in each survey round.

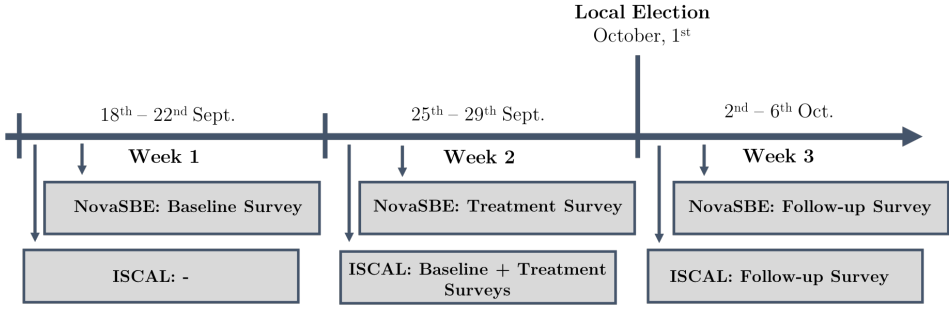
As in Galasso and Nannicini (2016), the treatment is administered in the final of the two-week official political campaign period, when politicians tend to invest great effort in campaigns.[67] Hence, it is likely that participants were exposed to other sources of information. It is important to recognize that our intervention constitutes a marginal source of information whose intensity may depend on several factors. It may happen that the information provided is not new to participants. In this regard, we adopted a intention-to-treat interpretation, i.e. regardless of whether participants fully adhere to the treatment.

[66]We do not intend to test the effectiveness of different delivering formats, but rather to test positive and negative-tone information conveyed in a similar format.

[67]The official campaign period starts two weeks before the election date and ends two days prior to the election, as defined by Law 1/2001 that regulates the electoral procedures for local government bodies.

Figure 3.1: Timeline of Experiment Implementation

This timeline describes the field work conducted between September 18th and October 6th, 2017.



3.2.3 Sampling and Randomization

The experiment took place in two business schools in Lisbon, Portugal (NovaSBE and ISCAL) between September 18th and October 6th 2017. We targeted both undergraduate and graduate (master-level) students. All surveys were administered in a classroom setting in a total of 71 classes.[68]

The surveys were filled in on paper, either at the beginning or at the end of each class, under the supervision of a member of the implementation team.[69] Participation was voluntary and anonymous, and there were no monetary incentives for participants. Respondents were asked to create a unique anonymous identifier at the beginning of the baseline survey, combining digits of their birth dates and phone number, which they could easily retrieve in the following rounds.[70] This ensures anonymity, while enabling researchers to link responses across the different survey rounds. This information was clearly stated in the survey cover page and was read by a member of the implementation team.

[68] At NovaSBE, each survey was implemented in a different round (one round a week, over 3 weeks). At ISCAL we collapsed the baseline and treatment surveys due to reasons related to the academic calendar.

[69] The implementation team was composed by the authors and some Ph.D. students who received specific training in order to adopt a standardized procedure.

[70] In total, the identifying code allows for 365 000 possibilities.

There are several advantages of conducting this experiment with university students through in-classroom implementation. Firstly, the opportunity cost of responding the survey is fairly low. This not only allows to collect more data (longer surveys), but also to obtain a higher response rate. The aggregated length of the surveys across the three rounds is around 30 minutes at NovaSBE and 20 minutes at ISCAL. Secondly, given that we target participants attending higher school programmes, we are confident in their ability to comprehend the written information content of the treatment. Moreover, a large percentage of students at universities in Lisbon are actually displaced students. This allows us to obtain heterogeneity in terms of birth places as well as voting constituencies. Finally, we expect a large percentage of first-time or low-experienced voters, for which the information may have a differential impact. In turn, the main caveat of our approach is that it does not rely on a representative sample of the population.

The randomization procedure was made at the individual level.[71] We organized the surveys by creating blocks of six surveys according to the following sequence: 1 control, 2 positive, 1 control and 2 negative-tone surveys (CPPCNN). Positive and negative treatment surveys were randomly drawn from the pool of different versions. These blocks were then distributed in classroom, row-by-row. Such a procedure aimed at ensuring adequate randomization (balancing) and minimizing contagion between treatment groups as a result of positive and negative surveys being answered side-by-side. This option comes at the expense of potential contagion from treatment to the control group. In any case, we tried to minimize this effect by requesting the class to keep silent during the surveys and by creating surveys with similar appearance.

[71]The alternative would be randomization by classroom. We discarded that for two reasons. Firstly, because of uneven class sizes, which would make it difficult to have balanced treatment and control groups. Secondly, our findings could capture the effect of spillovers among individuals within classroom and not the updated perception based on the information provided.

3.2.4 Estimation Strategy

We split our analysis into two parts, corresponding to the two research questions explained in Subsection 3.2.2. In order to evaluate the effect of information on the perception about central government performance, we ask the participants about their assessment of the central government performance after the treatment. The same question was also in the baseline survey, which allows to compute the change. We provided participants with a Likert-type scale ranging from "Very Unsatisfactory" to "Very Satisfactory", not allowing for neutral views.[72]

Besides the general government performance, we also include questions on specific policy areas. In the baseline survey, we ask about the perception of performance on specific policy areas under the jurisdiction of the central government. These include, but are not limited to, all the areas covered in the news articles. Then, in the treatment survey, we asked whether and to which extent the news articles changed perception about central government action regarding the specific policy area the article is about. The scale is 'Yes, improved a lot', 'Yes, improved', 'Did not change', 'Yes, worsened' and 'Yes, worsened a lot'. As an example, figure C4 shows how the question was presented in the survey version containing a road safety article. In a similar way, we ask about the impact of the information on the perception of the performance of the party in charge of the government (PS). The potential correlation between the two answers illustrates to which extent the opinions about the central government are tied to the performance perception of the party has a whole.

The main outcome variables in this first step are the perceptions on general and area-

[72]See figure C3. This scale was later converted to range from -1 (negative) to 1 (positive).

specific government performance. We estimate the following equation:

$$Performance\ Perception_i = \alpha + \beta_1 T.NEG_i + \beta_2 T.POS_i + \gamma X_i + \varepsilon_i \tag{3.1}$$

where $Performance\ Perception_i$ is the metric of performance reported by subject i; $T.NEG_i$ and $T.POS_i$ are indicator variables equal to one if respondent i belongs to the negative or positive treatment groups, respectively; and X_i is a vector of control variables, including socio-demographic and political variables.[73] In all regressions we include class dummies to account for class-specific effects associated with the implementation process. Standard errors are robust to heteroskedasticity.[74]

Then, we evaluate whether the exogenously driven perceived performance about central government impacts local election behavior. For that, we rely on the self-reported (retrospective) voting behavior inquired in the follow-up survey. The estimation strategy follows equation 3.1, where we replace the outcome variable with turnout and voting decision.

[73]Socio-demographic controls include dummies for university, gender, whether the student is living in an address different from his parents (displaced student), whether the respondent is allowed to vote for the first time in the 2017 local election, whether either one or both parents are unemployed and if the respondent identifies as Catholic. Additionally, we control for the age of the respondent, the number of household members and a measure of the household log-equivalent income. Political preferences are accounted for with a 3-level scale variable that measures the interest in politics, a dummy variable for whether the respondent reports their position in the political spectrum (left-right axis) and two additional variables that assume the value of one when reports a left-wing or centre position. We also include a dummy for the cases where respondents say that one of the reasons for which they position themselves in a given part of the spectrum is because their family and friends have that same ideology. Lastly, we also include two dummies that are associated with their political understanding and beliefs. The first is equal to one if they state that often politics are too complicated to understand and the other whenever they state that the person in power matters a lot for political and economic outcomes.

[74]Our results remain unchanged if we cluster standard errors at the class level.

3.3 Data

Our data consists of around 4800 individual survey responses in the three survey rounds. Keeping the individuals for whom we are able to link all surveys, we end up with a final sample of 1 800 participants. Around 27% are students at NovaSBE.[75] In each round, we implemented surveys in 71 classrooms, with a number of attendees ranging from 15 to 150.

In table C2, we present the percentage of responses by survey version. As expected given our CPPCNN randomization procedure, each of the three groups corresponds to around one third of the sample. We also perform balance tests on several baseline characteristics and find that groups do not differ from each other at 5% confidence level (table 3.1).

In table C3, we present the main descriptive statistics. A large majority of our sample is composed of undergraduate students, with an average age of 21.3 years. Although age ranges from 16 to 54 years, the distribution is skewed to the right. Unsurprisingly, a large majority of our sample is composed by inexperienced voters. Around 5% are under 18 (non-eligible to vote) in the 2017 election, and 47% are allowed to vote for the first time. Around 60% of the sample is female and 29% are displaced students. Although a substantial share of participants is originally from Lisbon region, we also have respondents from many other constituencies. In specific, our sample covers 170 out of 308 municipalities in Portugal. A spatial representation of voting areas (municipalities) is mapped in figure 3.2.

We also collected baseline information on respondents' political knowledge, interest, experience, and preferences (panels B, C and D of table C3). Party affiliation is reported

[75] We tested for differential attrition in both universities. We find no statistical differences on dropout rates. Similarly, we also do not find differences between the two treatment and the control groups. Results are available upon request.

Table 3.1: Balance Tests

This table presents a baseline balance test for some demographic variables across treatment and control groups. The last two rows present the p-value of a t-test for mean equality. The "T. Pos. vs. T. Neg." tests equality in means between positive and negative treatment groups, while the "T. vs. C." tests the difference between (polled) treatment and control groups.

Group	Female	ISCAL	Age	Displaced	HH income	Gov. perf. perc.
T. Pos	0.59	0.73	21.0	0.29	2530.4	0.16
T. Neg	0.60	0.74	21.7	0.29	2546.9	0.15
Control	0.57	0.74	21.2	0.29	2479.6	0.15
T. Pos. vs. T. Neg.	0.871	0.927	0.057	0.829	0.926	0.887
T. vs C.	0.344	0.959	0.646	0.979	0.704	0.772

by 6% of the respondents, while 26% report a low understanding of politics. Overall, 30.7% of the respondents identify as left or centre-left, 19% as centre and 48.3% as centre-right or right. Less than 2% report far-right or far-left ideologies.[76] We depict the distribution of respondents along the political spectrum for each school in figure 3.3.

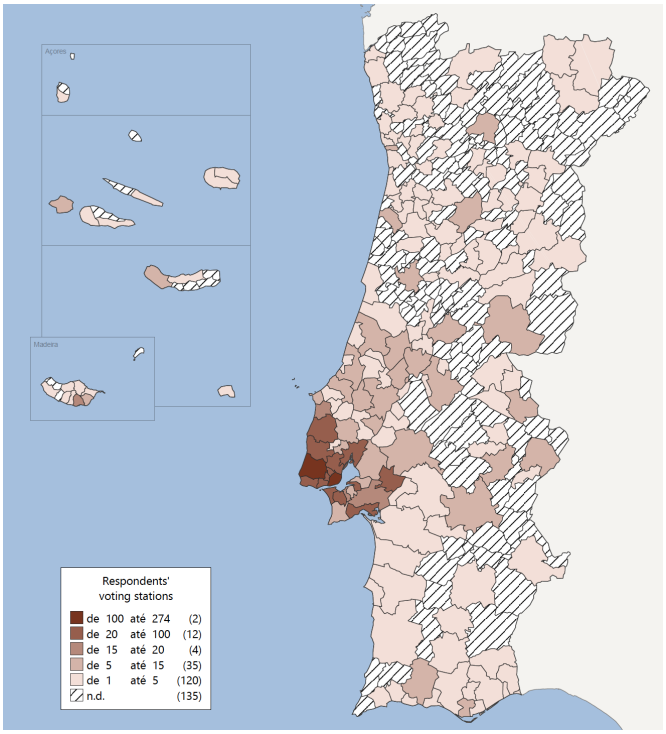
Most respondents are aware of the local election to be held soon, and 77% state in the baseline survey that they are willing to vote. We collected voting intention not only for the ones who intend to vote ('Which candidate do you plan to vote for?'), but also for the remaining ones ('In case you would vote, which candidate would you vote for with the highest probability?'). We specifically include the option "Prefer not to answer". After pooling the answers from these two groups, 23% do not know for whom they plan to vote. Another 23% plan to (or would) vote PS (the party in charge of the central government). The most common reason for abstention was distance from the voting station/constituency (36%).

Panel E shows summary statistics on performance perception. Regarding general government performance, the average is 0.15 on a scale from -1 (Very Unsatisfactory) to 1

[76]We asked respondents to self-locate on a left-right scale. We opted to focus on this standard classification and not ask about the conservative/liberal axis, given the over-representation of inexperienced voters.

Figure 3.2: Geographical Dispersion of Voting Areas (Municipalities)

This figure depicts the geographical dispersion of voting areas. Each voting area corresponds to a municipality. Different color intensities are associated with the number of respondents voting in each area. In parenthesis, we summarize the total number of municipalities in each respondent count bin.



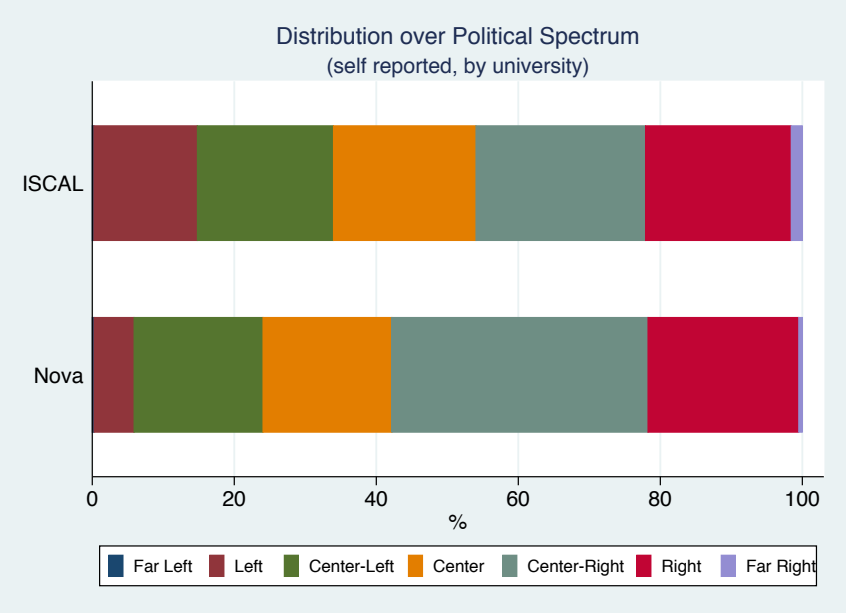
(Very Satisfactory) (statistically different from zero at 1% confidence level). This may reflect the government popularity at the time. As regards perception about specific areas, government is seen as being performing well in economy and public finance dimensions, and so does culture. Agriculture, forests and sea, together with health are rated negatively.[77]

In order to understand which policy areas drive the general baseline perception, we regress that on the perception of performance in specific areas. The results presented in table C4 show that economy and public finance are the most relevant dimensions. This result holds when we consider the sub-sample of ISCAL respondents, with a higher

[77]The negative result on agriculture, forests and sea is not surprising given the unprecedented forest fires in 2017, which spread due to a severe drought but also ineffective emergency response. There were 109 deaths and a four-fold increase in the burnt surface relative to the previous 10-year average.

Figure 3.3: Political Spectrum Distribution

This figure exhibits the self-reported political spectrum distribution of participants in the two business schools.



percentage of left-wing participants (column 2). In columns 3 and 4, we split the estimation by voters that in 2015 (last parliamentary elections) voted for PS or for the opposition parties (PSD + CDS). Again, economy and public finance stand out as key policy areas. Economic salience has been widely explored in the literature. Lewis-beck and Stegmaier (2000) argue, based on a survey of the literature, that voters generally weigh economic issues heavier than other issues when deciding on whom to vote for. More recently, Singer (2011a,b) discusses how economic salience fluctuates, particularly during financial crisis. It is not surprising that after the Portuguese sovereign debt crisis, respondents' opinion about the central government performance is mostly driven by the performance in economic-related areas.

Still in the baseline survey, we try to understand whether voting in local elections is somehow related to the central government. In particular, we asked respondents whether they consider using the vote in local elections to send a message to the gov-

ernment. One-third of the sample say so, and among those, 42% would send a positive message.

Next, we present descriptive statistics on realized voting outcomes. In the follow-up survey, 64% of the respondents state that they indeed voted. Official figures set the national turnout at 55%, which is lower than the self-reported turnout rate in our sample. This is consistent with self-reporting bias in post-election surveys due to misreporting of non-voters (Selb and Munzert, 2013). The gap in our experiment is however smaller than the average identified in that study.[78] Around 33% of our experiment's participants voted or would have voted for PS. According to official figures, PS received 38% of the votes in the 2017 election.

3.4 Perception on Central Government Performance

The treatment consists of an information bundle with a positive, neutral or negative-tone towards central government performance. It is intended to have induced exogenous variation in perceived performance. In this section we assess how participants' perception have been updated when exposed to the treatment.

Table 3.2 documents the immediate effect of information on self-reported perception. We start by showing the perception update in the specific policy area after each news article, both regarding the government (columns 1 and 3) and PS (columns 2 and 4). The direction of the effect goes as expected: participants tend to rate the government and PS positively after positive-tone news and do the opposite when exposed to negative-tone information. It is interesting to notice the strong correlation of answers concerning both the government and PS (0.65 for article 1, and 0.78 for article

[78]The authors compare survey turnout with actual turnout rates in 130 elections (covering 43 countries), and find an the average discrepancy of 13%, with a maximum of 42%.

2). Next, we evaluate whether the update in perceived performance in specific areas actually translates into the overall government performance assessment. Although all groups tend to rate the government positively, we document a monotonic increase in averages from negative treatment to control to positive treatment group (column 5). Yet, and although we have documented groups are comparable, this evaluation may be capturing underlying characteristics of each group. In order to tackle this issue, we can use the perception assessed in the baseline survey and compute the change. Column 6, shows the average change in general perception. On average, respondents that received the positive (negative) treatment improved (worsened) their perception about central government performance, with the negative impact being slightly larger in terms of magnitude. Both differences are statistically significant at 1% confidence level. We also find a slight upward adjustment among the control group, although the impact is only significant at 10% significance level. This may reflect events that happened in the week between the baseline and treatment rounds.[79]

We investigate further by estimating equation (3.1), where the outcome variable is the average perceived performance after the two news articles.[80] Results are shown in table 3.3. In line with table 3.2, we document a negative impact on specific performance perception among the negative treatment group as well as a positive impact among the positive treatment group. These results are robust to the inclusion of baseline government performance and additional controls related to political preferences.

Next, we evaluate the impact of the treatment on perceived overall central government performance (table 3.4). We observe a strong and significant effect of the negative treatment, corresponding to one-fourth of the standard deviation of the general govern-

[79]In fact, the positive impact among the control group is larger for NovaSBE sub-sample, where baseline and treatment surveys took place in subsequent weeks.

[80]Results are robust to considering only the last answer or when we create a panel of the two answers for each individual.

Table 3.2: Univariate Analysis of Government and PS Performance Perception After Treatment

This table presents a univariate analysis of the treatment effect. Here, we show how perception about performance changes after each news article. Government and PS performance are assessed through the question "Considering the information above, did your opinion change regarding the performance of the following entities [Government and PS]?". We then re-scaled the 5-node scale varying from "Yes, worsened a lot" to "Yes, improved a lot" to the interval [-1; 1]. General government performance (column 5) is a 4-level scale varying from "Very unsatisfactory" to "Very satisfactory" and re-scaled to fit the interval [-1;1]. Column 6 presents the change with respect to the baseline general government performance. Significance levels:* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Article 1		Article 2		Both articles	
	Government	PS	Government	PS	General Gov.	Δ General Gov.
	(1)	(2)	(3)	(4)	(5)	(6)
T. Positive	0.169	0.114	0.221	0.180	0.210	0.059***
T. Negative	-.009	-.0384	-0.084	-0.084	0.076	-0.081***
Control	-	-	-	-	0.182	0.026*

ment perception. This result is robust to the inclusion of controls related to political alignment. In fact, all those coefficients are positive and significant at least at 10% significance level, showing that voters aligned with the parties supporting the central government present a higher propensity to rate the government more positively. However, the coefficient on the positive treatment is no longer strong and is only statistically different from zero in a few specifications. These results are suggestive of negativity bias in the performance perception in our sample. Similar findings have been reported in the literature, such as in James and John (2007). In fact, although both negative and positive information affects respondents policy-specific perception, only negative news systematically affect the general performance perception of the central government.

3.4.1 Information and perception updating

In the previous section we documented that the treatment induced adjustment in self-reported perception and that this adjustment occurred in the expected direction. There are two potential concerns with the interpretation of these results.

Table 3.3: Treatment Effect on Policy-Specific Government Performance

This table presents the treatment effect on perception about policy-specific government performance. The outcome variable is the government performance perception associated with the policy areas covered in the information bundles. T.NEG. and T.POS. are dummy variables equal to 1 whenever respondents belong to negative and positive treatment, respectively. Centre and Left are dummy variables equal to one when respondents report to be in that part of the political spectrum. Vote PS and Vote PS+BE+CDU are dummy variables equal to one when respondents report an intention to vote for the respective parties in the baseline. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Policy-Specific Government Performance						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T. NEG.	-0.113***	-0.110***	-0.116***	-0.116***	-0.117***	-0.123***	-0.123***
	(-8.35)	(-8.23)	(-8.16)	(-8.16)	(-8.22)	(-7.56)	(-7.57)
T. POS.	0.131***	0.132***	0.117***	0.117***	0.117***	0.126***	0.126***
	(10.09)	(10.34)	(8.59)	(8.59)	(8.62)	(7.92)	(7.94)
Gov. perf. (Baseline)		0.065***	0.066***	0.066***	0.059***	0.067***	0.067***
		(4.62)	(4.52)	(4.54)	(3.98)	(3.95)	(3.97)
Center				-0.005			
				(-0.32)			
Left					0.028*		
					(1.82)		
Vote PS						0.026	
						(1.48)	
Vote PS+BE+CDU							0.027*
							(1.74)
Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	1736	1710	1503	1503	1503	1141	1141
adj. R-sq	0.156	0.164	0.155	0.154	0.156	0.177	0.178
F-test	5.322	5.261	-	-	-	-	-
p-value	0.000	0.000	-	-	-	-	-

The first relates to the fact that the treatment may be just amplifying a previous negative opinion about government actions in each policy area. We show this is not the case by interacting a variable indicating a baseline negative opinion on each area with negative treatment dummy (table 3.5). Indeed, the coefficient is non-significant.[81] Thus, irrespective of the controls included, we do not find any support for the hypo-

[81]Table 3.5 reports the results of a panel regression with two observations per individual: one associated with the answer to the policy-specific performance question appearing immediately after the first article and the other after the second article. We do so because each bundle covers two different topics for which there is a potentially different corresponding baseline opinion. Our findings do not change in a cross-sectional analysis.

Table 3.4: Treatment Effect on General Government Performance

This table presents the treatment effect on perception about general government performance. T.NEG. and T.POS. are dummy variables equal to 1 whenever respondents belong to negative and positive treatment, respectively. Centre and Left are dummy variables equal to one when respondents report to be in that part of the political spectrum. Vote PS and Vote PS+BE+CDU are dummy variables equal to one when respondents report an intention to vote for the respective parties in the baseline. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	General Government Performance						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
T. NEG.	-0.100***	-0.106***	-0.114***	-0.114***	-0.116***	-0.111***	-0.111***
	(-4.13)	(-6.17)	(-6.27)	(-6.26)	(-6.35)	(-5.36)	(-5.33)
T. POS.	0.033	0.033**	0.028	0.027	0.029*	0.037*	0.037*
	(1.47)	(1.98)	(1.61)	(1.58)	(1.69)	(1.82)	(1.84)
Gov. perf. (Baseline)		0.666***	0.661***	0.658***	0.646***	0.657***	0.661***
		(26.79)	(25.59)	(25.44)	(24.36)	(22.15)	(22.57)
Center				0.034*			
				(1.65)			
Left					0.064***		
					(3.62)		
Vote PS						0.062***	
						(3.36)	
Vote PS+BE+CDU							0.050***
							(2.86)
Controls	No	No	Yes	Yes	Yes	Yes	Yes
Obs.	1731	1708	1509	1509	1509	1150	1150
adj. R-sq	0.018	0.481	0.490	0.491	0.494	0.531	0.530
F-stat.	2.218	16.144	-	-	-	-	-
p-value	0.000	0.000	-	-	-	-	-

thesis that a negative baseline opinion makes participants updating their policy-specific performance perception differently when exposed to the treatment.

A second and more severe potential concern is that the observed impact may just reflect the intention of participants to please the researchers by behaving in the expected direction. While our experimental design does not allow to completely rule out this possibility, there are a couple of tests we can do to investigate it further.

The first argument against this channel is the evidence of negativity bias. If the observed impact purely reflects willingness to please the researchers, it is not clear why the impact

would be asymmetric. Yet, it can be the case that participants are more likely to behave in the expected direction when exposed to negative information, even if the information hasn't contributed to perception updating at all.

In order to investigate whether the information conveyed in the treatment is indeed an input of perception update, we can test whether participants with different ex-ante probability of having had access to that information in the past respond differently. In other words, if the treatment is indeed effective, we expect it generates larger adjustment when information is not only relevant but also new to the participant. We use two proxies for this *ex-ante* stock of information. Firstly we use self-reported interest in each policy area assessed in the baseline survey. Then, we use parents' occupation as an objective measure of access to information in different policy areas.

In table 3.5, we assess the marginal effect of treatment on individuals who declare to have high interest in the policy area covered in each news article.[82] For instance, for someone that was randomly assigned to an information bundle containing a negative-tone news article on the National Education System, we look whether the person has reported to have high interest in Education policy area in the baseline survey. As we interact the negative treatment with a 'High Interest' indicator, we find that an increased interest attenuates the overall treatment impact. A positive and significant sign of the interaction term suggests that the treatment effect is stronger among participants with lower levels of interest.

We also use parents' occupation as a proxy for access to information. In the baseline survey, a substantial percentage of the participants reveal to hold conversations about politics at home (45%) and it is one of the most important sources of political information, only surpassed by outdoor advertising and media (TV, radio, newspapers). As

[82]The question about interest in each area was just collected at NovaSBE, which justifies the drop in the number of observations.

Table 3.5: Treatment Effect On General Government Performance by *ex-ante* Opinion and Interest

This table presents the treatment effect on perception about policy-specific government performance. T.NEG. and T.POS. are dummy variables equal to 1 whenever respondents belong to negative and positive treatment, respectively. Negative Opinion and High Interest are dummy variables equal to 1 when respondent reports a baseline negative performance perception or a baseline high interest in the policy area of the information he or she was exposed to. Second article is equal to 1 whenever specific performance perception refers to an article presented as the second piece of information. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Post-Treatment Specific Government Performance					
	Opinion			Interest		
	(1)	(2)	(3)	(4)	(5)	(6)
T. NEG.	-0.237***	-0.237***	-0.228***	-0.346***	-0.342***	-0.330***
	(-8.68)	(-8.64)	(-7.87)	(-10.00)	(-9.68)	(-8.83)
T. NEG. x Negative opinion	-0.019	-0.017	-0.018			
	(-0.60)	(-0.54)	(-0.54)			
Negative opinion	-0.074***	-0.060**	-0.076***			
	(-3.23)	(-2.56)	(-3.06)			
T. NEG. x High Interest				0.131***	0.130**	0.118**
				(2.60)	(2.56)	(2.20)
High Interest				-0.021	-0.021	-0.014
				(-0.59)	(-0.58)	(-0.38)
Second article	-0.009	-0.008	-0.007	-0.060**	-0.057**	-0.053**
	(-0.66)	(-0.63)	(-0.49)	(-2.32)	(-2.21)	(-1.99)
Gov. perf. (Baseline)		0.064***	0.049**		0.076**	0.077**
		(3.40)	(2.39)		(2.46)	(2.15)
Controls	No	No	Yes	No	No	Yes
Obs.	2211	2193	1929	616	606	555
adj. R-sq	0.149	0.153	0.156	0.193	0.198	0.189
F-stat.	6.500	6.466	5.927	13.421	12.342	5.619
p-value	0.000	0.000	0.000	0.000	0.000	0.000

an example, participants whose parents are professors are expected to have *ex-ante* increased exposure to information related to Education. We test this hypothesis in table 3.6. In order to perform this exercise, we needed to search for parents' occupation directly related to some of the policy areas covered. We end up looking at teachers and school staff (Education) and doctors and nurses (Health). We also assess whether parent's employment status affects the reaction to negative news.

For all the occupations analyzed, we document a positive coefficient on the interaction

Table 3.6: Treatment Effect on General Government Performance by Awareness Level

This table presents the treatment effect on perception about policy-specific government performance. Here, we use parents occupation as a proxy for awareness in specific areas. The analysis in columns 1 to 4 is conditioned on policy areas that can be linked to parent's occupation (Education and Health). In columns 5 and 6 we include all surveys. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Post-Treatment Specific Government Performance					
	Education		Health		Unemployed Parents	
	(1)	(2)	(3)	(4)	(5)	(6)
T. NEG.	-0.312***	-0.259**	-0.319***	-0.314***	-0.254***	-0.247***
	(-3.70)	(-2.36)	(-9.63)	(-8.67)	(-15.98)	(-14.35)
T. NEG. x Occupation	0.196	0.194	0.205**	0.259**	0.060	0.075*
	(1.11)	(0.94)	(2.05)	(2.17)	(1.45)	(1.66)
Occupation	-0.099	-0.031	-0.217***	-0.265***	-0.032	-0.049*
	(-0.92)	(-0.20)	(-2.90)	(-3.15)	(-1.21)	(-1.75)
Gov. perf. (Baseline)		0.194		0.047		0.075***
		(1.59)		(1.10)		(3.52)
Controls	No	Yes	No	Yes	No	Yes
Obs.	135	118	411	365	1144	984
adj. R-sq	0.261	0.205	0.270	0.260	0.197	0.208
F-stat.	-	-	-	-	31.279	4.599
p-value	-	-	-	-	0.000	0.000

of negative treatment with parent's occupation indicator. Such a positive coefficient indicates a lower treatment effect among the group of participants that have presumably more information about the respective policy area. The coefficient is statistically different from zero at least at 10% significance level for Health and Unemployed Status. This result is consistent with the previous findings using self-reported interest.

In this subsection we documented that the response to information is different across different characteristics related to the *ex-ante* stock of information. These results suggest that the reported treatment effect found above is not driven by the willingness of participants to please the researchers by behaving in the expected direction.

Table 3.7: Treatment Effect on General Government Performance among Undecided and First Election Voters

This table presents the treatment effect on perception about policy-specific and general government performance. The analysis is conditioned on the sub-sample of participants that are either undecided regarding whom to vote for in the baseline survey or are allowed to vote for the first time in the 2017 local election. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Undecided Voters				First Election			
	Specific Gov. Perf.		General Gov. Perf.		Specific Gov. Perf.		General Gov. Perf.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T. NEG.	-0.116***	-0.119***	-0.092***	-0.117***	-0.114***	-0.133***	-0.055*	-0.080***
	(-6.31)	(-6.35)	(-2.79)	(-4.98)	(-6.55)	(-7.05)	(-1.67)	(-3.36)
T. POS.	0.124***	0.108***	0.011	0.027	0.126***	0.112***	0.068**	0.067***
	(7.27)	(6.12)	(0.36)	(1.15)	(7.31)	(6.09)	(2.17)	(3.02)
T. NEG. x Undecided	-0.038	-0.017	-0.021	0.028				
	(-1.02)	(-0.45)	(-0.33)	(0.54)				
T. POS. x Undecided	0.049	0.085**	0.092	0.043				
	(1.28)	(2.09)	(1.56)	(0.98)				
Undecided	0.028	-0.013	0.037	-0.009				
	(1.15)	(-0.56)	(0.84)	(-0.30)				
T. NEG. x First Election					0.004	0.036	-0.109**	-0.085**
					(0.15)	(1.24)	(-2.25)	(-2.25)
T. POS. x First Election					0.010	0.014	-0.084*	-0.090**
					(0.38)	(0.48)	(-1.89)	(-2.56)
First Election					0.003	-0.006	0.064*	0.062**
					(0.13)	(-0.27)	(1.75)	(2.02)
Gov. perf. (Baseline)		0.065***		0.652***		0.058***		0.636***
		(3.88)		(22.28)		(3.86)		(23.66)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	1296	1141	1297	1150	1735	1503	1730	1509
adj. R-sq	0.173	0.181	0.016	0.533	0.154	0.155	0.020	0.499
F-stat.	4.686	-	4.534	-	5.097	-	2.354	-
p-value	0.000	-	0.000	-	0.000	-	0.000	-

3.4.2 Undecided and Inexperienced Voters

Our sample is composed to a large extent by participants with low voting experience. It is interesting to analyze the impact of the treatment according to this dimension. Firstly, we look at participants that were undecided regarding whom to vote for in the baseline round week (Undecided Voters). Second, we analyze the subset of first

time voters in our sample (First Election). These respondents were identified based on their reported birth date. In both cases, the reaction to the treatment may differ from the overall sample. Undecided voters may be more sensitive to the information provided, because they may be less informed or confused about the information they have received. First time voters, in turn, have no retrospective voting habits and thus may be more sensitive to (new) information. On the other hand, they may also be less engaged with politics and thus be less informed, which could amplify the informational power of the treatment.

Table 3.7 shows the heterogeneous effects of treatment on the sub-sample of undecided voters and first time voters. Undecided voters do not seem to react in a systematically different way to the information provided.

We observe a similar result when looking at respondents eligible to vote for the first time. Columns 5 and 6 show that this sub-sample does not react differently to the treatment in policy-specific areas. However, this is the group that drives the strong negativity bias found in general government performance: negative information translates strongly to the aggregate perception about government performance. The effect of positive information is more diluted than for the whole sample (columns 7 and 8).

3.5 Central Government Performance and Local Election Outcomes

The previous section provides evidence that the treatment had an effect both on the perception about the incumbent central government and about PS. We now address the second research question, i.e., whether the exogenously-induced update in perception affects local election outcomes.

3.5.1 Turnout

We start by analyzing the impact of the treatment on electoral turnout, which is shown in table 3.8. We have no prior hypothesis about the direction of the effect concerning turnout. On the one hand, information can keep individuals away from the polls if it harms their trust in politics or politicians. On the other hand, it may increase the desire to vote, whenever the vote is perceived as an important tool to change the *status quo*.

Regardless of the specification, we find no impact of the treatment on the decision to cast a vote. The stronger predictor of post-election reported turnout is the expected turnout reported in the baseline survey. Interestingly, the perception about central government performance does not systematically increase or decrease the decision of voting.

3.5.2 Voting Outcome

We now focus our attention on the chosen candidate. In the following tables we aggregate the reported choices of voters and non-voters (remember we asked both in which candidate they did vote for or would have voted for if they had cast a vote). Our results remain unaltered if we take the two groups separately. Results are shown in table 3.9.

Due to the political configuration of the central government in charge at the time of the election, we aggregate the reported voting choices for candidates from the parties supporting the government (PS, BE and CDU) and consider all other candidates (from PSD, CDS, other smaller parties or independent candidates) jointly, which we label "Other parties".[83] Presumably, positive (negative) information would benefit (harm) the local candidates aligned with the parties supporting the central government and

[83]Considering only the parties represented in the national Parliament does not change our results.

Table 3.8: Treatment Effect on Turnout

This table presents the treatment effect on turnout. The outcome variable is a dummy variable equal to one when respondent reports to have voted in the election. Turnout (baseline) refers to the intention to vote reported in the baseline survey. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Some specifications include voting area dummies (municipalities). Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Turnout		
	(1)	(2)	(3)
T. NEG.	-0.026	-0.026	-0.012
	(-1.18)	(-1.12)	(-0.49)
T. POS.	-0.026	-0.032	-0.033
	(-1.17)	(-1.38)	(-1.32)
Turnout (Baseline)	0.688***	0.651***	0.626***
	(34.11)	(25.64)	(21.60)
Gov. perf. (Baseline)	0.014	-0.003	0.013
	(0.62)	(-0.13)	(0.50)
Controls	No	Yes	Yes
Municipal Dummies	No	No	Yes
Obs.	1768	1547	1545
adj. R-sq	0.381	0.407	0.423
F-stat.	46.467	-	-
p-value	0.000	-	-

harm (benefit) all other candidates.

Overall, there is no treatment effect on voting outcomes. We find no significant impact of the treatment on the likelihood of voting for the parties supporting the central government (PS + BE + CDU), for the opposition parties (PSD + CDS) or casting a blank ballot. Regardless of the outcome variable, voting intentions are highly persistent as the baseline voting plan explains realized voting outcomes to a large degree. Unsurprisingly, a higher baseline government performance perception has a positive impact on voting for the parties supporting the central government (columns 1 to 3), and reduced the likelihood of voting for opposition parties (columns 4 to 5). The effect on the likelihood of a blank ballot is non significant.

Our results seem to suggest that central government performance does not play a key

Table 3.9: Treatment Effect on Voting Decisions

This table presents the treatment effect on voting decision. The outcome variable is a dummy variable equal to one when respondent reports to have voted (or would have voted had he or she voted) in any of the parties. We aggregate votes of government supporting parties (PS+BE+CDU), other parties (including the opposition parties PSD and CDS), and blank ballots. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Some specifications include voting area dummies (municipalities). Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Voting decision								
	PS+BE+CDU			Other parties			Blank Ballot		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T. NEG.	0.018	0.015	-0.001	0.012	0.007	0.017	-0.008	-0.005	-0.000
	(0.69)	(0.56)	(-0.02)	(0.47)	(0.28)	(0.59)	(-0.40)	(-0.27)	(-0.01)
T. POS.	0.012	0.005	-0.006	0.021	0.007	0.013	-0.003	0.006	0.012
	(0.43)	(0.16)	(-0.19)	(0.81)	(0.28)	(0.44)	(-0.13)	(0.31)	(0.53)
PS+BE+CDU (Baseline)	0.606***	0.566***	0.516***						
	(24.02)	(19.35)	(14.68)						
Other Parties (Baseline)				0.634***	0.558***	0.500***			
				(24.82)	(17.81)	(13.74)			
Blank Ballot (Baseline)							0.602***	0.556***	0.537***
							(14.64)	(12.30)	(10.65)
Gov. perf. (Baseline)	0.196***	0.139***	0.147***	-0.159***	-0.109***	-0.120***	0.001	-0.023	-0.019
	(7.64)	(5.36)	(5.01)	(-6.28)	(-4.18)	(-4.22)	(0.07)	(-1.14)	(-0.82)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipal Dummies	No	No	Yes	No	No	Yes	No	No	Yes
Obs.	1191	1074	1074	1191	1074	1074	1191	1074	1074
adj. R-sq	0.427	0.466	0.465	0.475	0.505	0.524	0.342	0.355	0.350
F-stat.	28.996	87.277	-	40.070	104.803	-	7.038	14.723	-
p-value	0.000	0.000	-	0.000	0.000	-	0.000	0.000	-

role as a determinant of local election outcomes. Indeed, it may be that voters do not benefit/penalize local candidates based on their perception about the central government performance. This could be seen as evidence that local elections are not second order elections and would be consistent with Marien et al. (2015), whose main conclusion is that "local candidates contribute strongly to the salience of electoral decisions on the local level". However, the nonexistent treatment effect can also be due to treatment dilution. Due to unobserved characteristics such as prior exposure to information or the time devoted to read and understand the information bundle, there may be problems of non-compliance or protocol deviations. As explained in Angrist (2006), regressing the

outcome variable directly on treatment assignment will provide conservative estimates.

The solution is to use the treatment assignment as an instrument for the actual treatment – in our case, the updated perception of government performance –, and then estimating the impact of the instrumented variable on the outcome variable. Using the IV methodology requires the exclusion restriction to be satisfied though, meaning that the only way through which the treatment has an impact on voting outcomes is actually through the updated perception. While we cannot claim it holds, we perform this estimation as a robustness exercise.

We have shown the first stage of the two-stage least square estimation (impact of treatment on perception) in section 3.4. The second stage corresponds to the following equation:

$$Voting\ Outcome_i = \alpha + \beta \widehat{Performance\ Perception_i} + \gamma X_i + \eta_i \tag{3.2}$$

where $Voting\ Outcome_i$ is the outcome variable of interest. This methodology isolates the impact of an exogenously treatment-induced change in government performance perception on the probability of voting for a given candidate. For sake of brevity we show only the estimates of the likelihood of voting for PS.

The second stage results are presented in table 3.10. All specifications control for baseline government performance perception and the initial plan to vote for PS reported in the baseline survey. In the first two columns, we use positive treatment and negative treatment indicators as instruments. In the remaining columns, we use all individual survey version indicators as instrumental variables.

We find positive but non-significant coefficients on the instrumented variable in the

Table 3.10: Second Stage Regression (2SLS) of Perceived Central Government Performance on Local Election Decisions

This table presents the (second stage) regression coefficients of the instrumented perceived central government performance on local election voting decision. The outcome variable is a dummy variable equal to one when respondent reports to have voted in PS. Columns 1 and 2 report the estimates when we use positive treatment, negative treatment and control assignment as instrumental variables. Columns 3 and 4 use all survey versions as instruments. Gov. Perf. (post-treatment) denotes the(instrumented) government performance perception from the first stage. Baseline government performance perception is a 5-level scale re-scaled to the interval [-1;1]. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Vote for PS			
	IV: T. Pos. & T. Neg.		IV: Survey Dummies	
	(1)	(2)	(3)	(4)
Gov. perf. (post-treatment)	0.223	0.203	0.293**	0.312*
	(1.29)	(1.02)	(1.87)	(1.92)
Vote PS (Baseline)	0.589***	0.589***	0.565***	0.564***
	(16.02)	(15.22)	(15.87)	(14.93)
Gov. perf. (Baseline)	0.015	0.036	-0.033	-0.030
	(0.13)	(0.27)	(-0.32)	(-0.28)
Controls	No	Yes	No	Yes
Obs.	1166	1051	1166	1166
Cent. R Sq.	0.3734	0.3841	0.3659	1051
F-stat	229.22	65.54	220.13	60.58
p-value	0.000	0.000	0.000	0.000

first two columns. The results turn out significant when we use all survey version as instrumental variables. However, the results are only significant at 10% significance level. Moreover, given that the F-test of the excluded instruments is below 10 in this case, we believe this approach strengthens our central argument of a lack of average treatment effect in the overall sample.

3.5.3 Undecided and Inexperienced Voters

We found evidence that central government performance perception does not impact local voting behavior in the overall sample. Nevertheless, we can investigate whether the response is different among the group of undecided voters. The interest of analyzing

this group lies on the fact that the information participants were exposed to may be residual to the overall sample, but it can be relevant for undecided or swing voters. Recall that these respondents did not react differently to the information provided nor they use it in a different way in the perception updating process, as shown in section 3.4.2.

In table 3.11 we extend the main specification with interaction variables between treatment and undecided voters indicator. When analyzing the effect of negative treatment on this sub-sample, we observe a negative effect on the probability of voting in the supporting parties (PD + BE + CDU). This coefficient is non-statistically significant though. In turn, there is a statistically significant increase in the vote of opposition parties ('Other parties'). The impact is sizable. The likelihood of an undecided voter to vote in an opposition party increases by 16 to 22 percentage points when exposed to negative information about the central government performance.

We also evaluate the effect on blank ballot voting. It decreases as a response to the negative treatment for this sub-sample. When we look at the size of the coefficients, there seems to be an overall vote transfer from PS + BE + CDU as well as from blank ballot to the opposition parties.

We next investigate the effect of the positive treatment. Unlike the negative treatment, the positive treatment did not have a strong effect on perception updating (section 3.4). Therefore, it is not surprising that we do not find a strong significant impact of this treatment on voting outcomes, neither at the full nor at the sub-sample of undecided voters. Nevertheless, the positive treatment decreases the likelihood of blank ballot by 14 to 15 percentage points. The blank votes seem to have been distributed more in favor of the opposition parties, although it is only statistically significant in column 5.

We performed a similar analysis for respondents that are allowed to vote for the first

Table 3.11: Treatment Effect on Voting Decision among Undecided Voters

This table presents the treatment effect on voting decision. The analysis is conditioned on the sub-sample that reports to be undecided regarding on whom to vote for in the baseline survey. The outcome variable is a dummy variable equal to one when respondent reports to have voted (or would have voted had he or she voted) in any of the parties. We aggregate votes of government supporting parties (PS+BE+CDU), other parties (including the opposition parties PSD and CDS), and blank ballots. Additional controls include socio-demographic and measures of engagement/interest in politics. All regressions include class dummies. Some specifications include voting area dummies (municipalities). Standard errors are robust and t-statistics are reported in parenthesis. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Voting decision among undecided voters								
	PS+BE+CDU			Other parties			Blank Ballot		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T. NEG.	0.018	0.030	0.012	-0.020	-0.035	-0.028	0.012	0.012	0.019
	(0.67)	(1.15)	(0.41)	(-0.80)	(-1.46)	(-1.01)	(0.67)	(0.71)	(0.93)
T. POS.	-0.010	-0.005	-0.022	-0.012	-0.025	-0.016	0.022	0.030	0.038*
	(-0.38)	(-0.19)	(-0.71)	(-0.49)	(-0.98)	(-0.53)	(1.14)	(1.53)	(1.68)
T. NEG. x Undecided	-0.038	-0.111	-0.101	0.164**	0.228***	0.224***	-0.123*	-0.107	-0.118*
	(-0.45)	(-1.30)	(-1.11)	(2.08)	(2.86)	(2.72)	(-1.83)	(-1.59)	(-1.73)
T. POS. x Undecided	0.050	0.012	0.040	0.128	0.150*	0.118	-0.164**	-0.147**	-0.151**
	(0.61)	(0.14)	(0.44)	(1.63)	(1.83)	(1.42)	(-2.54)	(-2.25)	(-2.16)
Undecided	0.267***	0.276***	0.261***	0.167***	0.146***	0.144**	0.255***	0.204***	0.187***
	(4.54)	(4.46)	(4.02)	(3.10)	(2.69)	(2.53)	(4.98)	(3.99)	(3.47)
PS+BE+CDU (Baseline)	0.699***	0.653***	0.607***						
	(27.05)	(21.28)	(16.48)						
Other Parties (Baseline)				0.722***	0.647***	0.594***			
				(28.75)	(20.83)	(16.05)			
Blank Ballot (Baseline)							0.636***	0.588***	0.566***
							(15.76)	(13.01)	(11.21)
Gov. perf. (Baseline)	0.147***	0.110***	0.118***	-0.143***	-0.099***	-0.108***	-0.006	-0.025	-0.020
	(5.84)	(4.24)	(4.08)	(-5.87)	(-3.96)	(-3.96)	(-0.33)	(-1.30)	(-0.91)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Municipal Dummies	No	No	Yes	No	No	Yes	No	No	Yes
Obs.	1191	1074	1074	1191	1074	1074	1191	1074	1074
adj. R-sq	0.467	0.495	0.495	0.516	0.547	0.563	0.380	0.377	0.367
F-stat.	33.217	85.039	-	40.722	111.767	-	6.875	14.275	-
p-value	0.000	0.000	-	0.000	0.000	-	0.000	0.000	-

time. We find no differential response to treatment among this sub-sample. Results are available upon request.

To sum up, we do not observe a significant treatment effect of our experiment. This suggest that performance perception about central government is not a key driver of voting at local elections. However, we find some support to the idea that voters with

less rigid voting preferences, such as the ones who are undecided very close to the election date, resort to their updated performance perception to shape their local voting behavior. These findings are aligned with those of Epstein and Robertson (2015), who show that search engine manipulation, simply by biasing the search result rankings "can shift the voting preferences of undecided voters by 20%".

3.6 Conclusion

We conducted a randomized controlled trial to evaluate, in a causal framework, the impact of perception on central government performance and local election behavior. Firstly, the research design allows us to measure the different impact of positive and negative-tone information on the perception updating about the performance of central government. Secondly, we measure to which extent the updated perception implies a different behavior in local elections.

Our experiment was carried out in Portugal in the weeks around the 2017 local election. This research was conducted with 1 800 students from two business schools in Lisbon. The treatment consisted of exposing participants to factual information about central government performance in several policy areas. In total, we created 13 different information bundles that cover diverse policy areas such as public finance or road safety. Besides the large sample, our work distinguishes from previous studies by using comparable positive and negative bundles of information, no use of fictional information and diversification of information topics.

We document that participants react to information and use it to update their perception about central government performance. The adjustment in perception suggests an asymmetrical effect of positive and negative information: negative treatment induces

a downward revision on the general perception about central government performance, while the positive treatment has a smaller and frequently non-significant effect. This phenomenon (negativity bias) has been previously documented in the literature. The comprehensive information we gathered about our respondents allows us to further understand the updating process. As we show, the *ex-ante* stock of information plays a role: the negativity bias is lower among participants with higher levels of (political) awareness and higher for less experienced voters. These results reinforce the belief that information provision, particularly with a negative tone, affects the perception about political performance. This finding can be of particular importance in the context of *fake news* and negative advertising.

Next, we investigate whether and how the exogenously-induced change in perception about government performance impacts on local election behavior. The existing literature on second order elections shows that electoral behavior is sometimes driven by factors that are related to other political administration layers. With the exception of Marien et al. (2015), this literature has mostly focused on how voting in European elections is affected by the developments of national politics of member states. Moreover, voters may see the party system as a selection mechanism for local candidates. Thus, the performance of the central government may signal local candidates' quality.

We focus on turnout and voting decisions as local election outcomes. Our findings show that there is no average treatment effect of central government performance perception on neither of those. These results can be interpreted in two ways. On the one hand, they may suggest that local election behavior is not fundamentally influenced by central political developments. On the other hand, it may arise due to a treatment dilution problem. We circumvent this problem by using an instrumental variable approach. Still, the evidence of the impact is weak.

Importantly, we find some support to the idea that voters with less rigid voting preferences, such as the ones who are undecided very close to the election date, resort to their updated performance perception to shape their local voting behavior. These findings are aligned with those of Epstein and Robertson (2015), who show that search engine manipulation, simply by biasing the search result rankings "can shift the voting preferences of undecided voters by 20%".

Appendix of Chapter 3

Table C1: Survey Contents

This table describes the contents of each survey round. Panel A shows the information collected in each survey. Panel B describes the information bundles in the 13 survey versions.

Panel A: Information collected in each survey round

Baseline	Treatment	Follow-up
• Sociodemographics, political orientation, interest in politics, past voting experience; • *Expected* voting behavior*; • Policy-specific and general perception of current government performance.	• *Expected* voting behavior*; • Positive, neutral and negative information bundles; • *Updated* voting behavior ; • *Updated* perception of current government performance.	• *Realized* voting behavior*.

*Voting Behavior includes: turnout, voting direction (candidate) and reasons, for both voters and non-voters.

Panel B: Information bundles

	Survey No.	1st Article	2nd Article
Neg/Pos	1/7	Public Finance	Pensions
	2/8	Public Finance	Youth Employment
	3/9	Public Finance	Road Safety
	4/10	National Health Service	Youth Employment
	5/11	National Health Service	Education
	6/12	National Health Service	Road Safety
C	13	Portuguese Dog Breed	

Table C2: Percentage of respondents by survey type and treatment groups

This table shows the percentage of responses by each survey type and treatment groups.

Treat. Group	Survey No.	% Survey Type	% Treat. Group
T. Neg	1	5.83	32.94
	2	5.50	
	3	5.00	
	4	5.67	
	5	5.11	
	6	5.83	
T. Pos	7	5.67	33.28
	8	5.83	
	9	5.89	
	10	4.83	
	11	5.22	
	12	5.38	
Control	13	33.78	

Table C3: Descriptive Statistics

This table presents summary statistics.

	Obs.	Mean	S.D.	Min.	Max.
Panel A: Socio-Demographic Characteristics					
ISCAL	1 800	0,73	0,44	0	1
Undergraduate	1 797	0,97	0,16	0	1
Female	1 797	0,59	0,49	0	1
Age	1 784	21,32	5,97	16	54
Under legal age to vote until last elections (2016)	1 785	0,47	0,5	0	1
Household size	1 728	3,59	1,15	1	11
HH income per capita (equiv.)	1 710	1352,72	1631,49	111,8	17677,7
Catholic	1 778	0,55	0,5	0	1
At least one parent is unemployed	1 800	0,16	0,37	0	1
At least one parent is civil servant	1 761	0,27	0,44	0	1
Panel B: Turnout determinants and knowledge					
Displaced student	1 793	0,29	0,45	0	1
Person in power makes difference	1 786	0,54	0,50	0	1
One vote makes difference	1 789	0,60	0,49	0	1
Understanding politics is complicated	1 780	0,17	0,38	0	1
Panel C: Interest and experience in politics					
Self-reported interest in politics (1: No interest, 4: very interested)	1 789	2,6	0,78	1	4
Belongs to a political party as militant	1 775	0,06	0,24	0	1
Actively supported a candidate in this local election	1 794	0,06	0,23	0	1
General interest in this election (1: No interest, 4: very interested)	1 794	2,51	0,88	1	4
Panel D: Political preferences and opinion (general)					
Knows which part of the political spectrum identifies with	1 772	0,72	0,45	0	1
Position in the spectrum results from own ideas	1 717	0,61	0,49	0	1
Position in the spectrum results from family and friend contexts	1 717	0,4	0,49	0	1
Position in the spectrum results from opinion about politicians	1 717	0,22	0,41	0	1
Within the same party politicians share ideology no matter central or local roles	1 758	0,49	0,5	0	1
Panel E: Opinion towards current central government					
General gov. performance (-1: negative, 1: positive)	1 769	0,15	0,4	-1	1
Gov. performance (-1: negative, 1: positive): agriculture, forest and sea	1 731	-0,28	0,61	-1	1
Gov. performance (-1: negative, 1: positive): home affairs	1 714	-0,04	0,58	-1	1
Gov. performance (-1: negative, 1: positive): culture	1 726	0,26	0,59	-1	1
Gov. performance (-1: negative, 1: positive): defense	1 711	-0,09	0,58	-1	1
Gov. performance (-1: negative, 1: positive): education	1 722	0,09	0,69	-1	1
Gov. performance (-1: negative, 1: positive): economy	1 727	0,32	0,68	-1	1
Gov. performance (-1: negative, 1: positive): public finance	1 719	0,23	0,67	-1	1
Gov. performance (-1: negative, 1: positive): health	1 721	-0,17	0,62	-1	1
Gov. performance (-1: negative, 1: positive): labor and social security	1 722	0,09	0,63	-1	1
Considers using the vote to send a message to gov.	1 761	0,33	0,47	0	1
Considers sending a positive message to gov.	549	0,42	0,49	0	1
Panel F: Voting behavior in Local Elections 2017					
Knows about election	1 788	0,95	0,21	0	1
Intends to vote	1 793	0,77	0,42	0	1
No age to vote	1 793	0,05	0,22	0	1
Does not know on whom to vote for	1 332	0,23	0,42	0	1
Intends to vote for PS	1 332	0,23	0,42	0	1
Why plan not to vote (2017): I do not identify with electoral programs	312	0,21	0,41	0	1
Why plan not to vote (2017): I do not care	312	0,25	0,43	0	1
Why plan not to vote (2017): I was far from my voting area	312	0,36	0,48	0	1
Voted (follow-up)	1 798	0,64	0,48	0	1
Did not vote (follow-up)	1 798	0,3	0,46	0	1
No age to vote (follow-up)	1 798	0,05	0,22	0	1
Voted/would vote in PS (gov. party)	1 453	0,33	0,47	0	1
Did you consider voting in a different candidate?	1 143	0,27	0,44	0	1
In your decision, which was the most important: candidate	1 097	0,51	0,5	0	1
In your decision, which was the most important: party	1 097	0,4	0,49	0	1

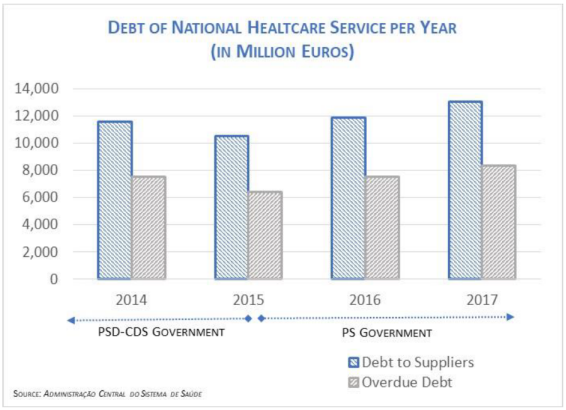
Table C4: Relation between General Gov. Performance and Performance in Different Policy Areas (Baseline)

This table presents the relation between (self-reported) general government performance and government performance in specific policy areas. The outcome variable is the general government performance perception, a 5-level scale re-scaled to the interval [-1;1]. Dependent variables are performance perception in specific policy areas, a 3-level scale re-scaled to the interval [-1;1]. Both independent and dependent variables are measured in the baseline survey. In each column, we report the results on a different sub-sample. Columns 3 and 4 correspond to the sub-sample of voters that have voted PS or Opposition parties in the previous central government elections (2015). Standard errors are clustered at class level, t-statistics are reported in parenthesis. Significance level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	General government performance (baseline)			
	All	ISCAL	PS (2015)	Opposition (2015)
	(1)	(2)	(3)	(4)
Agriculture, forest and sea	0.039***	0.039**	0.047	0.045
	(2.90)	(2.57)	(1.16)	(1.15)
Home affairs	0.069***	0.066***	-0.021	0.054
	(4.69)	(3.99)	(-0.44)	(1.33)
Culture	0.021	0.024	0.061	0.044
	(1.46)	(1.51)	(1.23)	(1.14)
Defense	0.044***	0.047***	0.043	0.028
	(3.02)	(2.85)	(0.82)	(0.64)
Education	0.046***	0.036**	-0.072	0.051
	(3.60)	(2.52)	(-1.63)	(1.41)
Economy	0.157***	0.144***	0.175***	0.256***
	(9.08)	(7.38)	(3.01)	(5.23)
Public finance	0.115***	0.108***	0.071	0.127***
	(6.68)	(5.47)	(1.26)	(2.63)
Health	0.028**	0.013	-0.014	0.060
	(1.97)	(0.83)	(-0.32)	(1.52)
Labor and social security	0.074***	0.076***	0.096**	0.050
	(5.12)	(4.83)	(2.01)	(1.24)
Obs.	1621	1213	128	203
Adj. R Sq.	0.360	0.342	0.192	0.500
F-stat.	102.328	70.986	4.361	23.480
p-value	0.000	0.000	0.000	0.000

Figure C1: Example of negative treatment information bundle (survey 6)

Part I: Health



"PSD puts pressure in the accounts of Ministry of Health"

(...)

According to PSD, "a good example of this concerning reality is the debt of the National Healthcare Service to the pharmaceuticals that increased, just from June to July 2016, more than 13 million euros"

Adapted from *Expresso*, September 24th, 2017

Part II: Road Safety

"ACP argues that road safety is 'not a priority' to the Government"

(...)

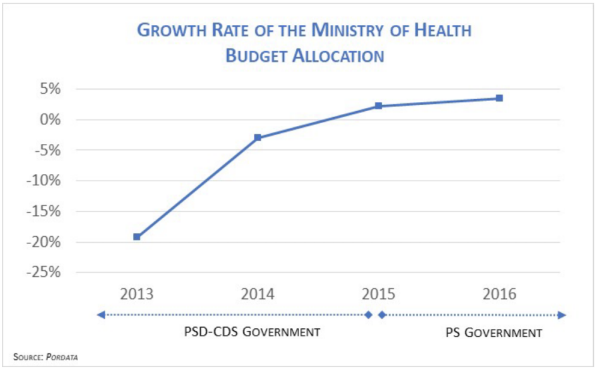
"Road accidents worrisome increased this year, without the Government understanding the actual problem or taking adequate structural measures to avoid this scourge", states ACP, in a press release. For the association, "road safety is still not considered a national priority" and Portugal "is still above the European average in terms of road accidents and the Government does nothing to reverse this fact".

(...)

Adapted from *Diário de Notícias*, July 3rd, 2017

Figure C2: Example of positive treatment information bundle (survey 12)

Part I: Health



"Ministry of Health with juiced up budget"

The global budget of the Ministry of health will increase by 353,3 million euros in 2017. Of which 156 million euros represent an increase of the budget allocation of the national healthcare service (around 2%). The minister of health is satisfied with the budget, that will create conditions to satisfy the expected increase in the demand for health services in 2017, as well as an increase in infrastructure investment.
(...)
Adapted from *Expresso*, October 14th, 2016

Part II: Road Safety

"Portugal has a new program to prevent driving under the influence of alcohol"

The new National Strategic Plan of Road Safety includes 108 measures that intend to "change the landscape of road safety in Portugal"
(...)
According to the new plan, until 2020 several awareness campaigns are going to be implemented and the security of the roads is going to be classified and inspected

Adapted from *Diário de Notícias*, April 20th 2017

Figure C3: General Government Performance Question

This figure shows the question on general government perception (translated to English). This question kept the same format in baseline and treatment surveys.

D.9 How do you evaluate the performance of current government (Partido Socialista) and the Prime-Minister António Costa?

- ☐ Very unsatisfactory
- ☐ Unsatisfactory
- ☐ Satisfactory
- ☐ Very satisfactory

Figure C4: Policy-Specific Governmemt Performance Question

This figure shows the question on area-specific government perception (translated to English). This question was included in the treatment survey after each of the news articles. In this case, we exhibit the one following a news article on Road Safety.

C.I Considering the information above, did your opinion change regarding the performance of the following entities in the **Road Safety** scope?

	Yes, improved a lot	Yes, improved	Did not change	Yes, worsened	Yes, worsened a lot
Government	❍	❍	❍	❍	❍
Partido Socialista	❍	❍	❍	❍	❍

Bibliography

Allcott, H. and Gentzkow, M. (2017). Social Media and Fake News in the 2016 Election. *Journal of Economic Perspectives*, 31(2):211–236.

Amiti, M. and Weinstein, D. E. (2018). How much do idiosyncratic bank shocks affect investment? evidence from matched bank-firm loan data. *Journal of Political Economy*, 126(2):525–587.

Anderson, C. D. (2006). Economic Voting and Multilevel Governance: A Comparative Individual-Level Analysis. *American Journal of Political Science*, 50(2):449–463.

Anderson-Macdonald, S., Chandy, R., and Zia, B. (2014). The impact of marketing (versus finance) skills on firm performance: Evidence from a randomized controlled trial in south africa. In *STICERD Economics Seminar. Retrieved from http://www. haas. berkeley. edu/groups/marketing/seminars/Papers/Fall*, volume 202015.

Angrist, J. D. (2006). Instrumental variables methods in experimental criminological research: What, why and how. *Journal of Experimental Criminology*, 2(1):23–44.

Ansolabehere, S., Iyengar, S., Simon, A., and Valentino, N. (1994). Does Attack Advertising Demobilize the Electorate? *American Political Science Review*, 88(4):829–838.

Arceneaux, K. and Nickerson, D. W. (2010). Comparing negative and positive cam-

paign messages: Evidence from two field experiments. *American Politics Research*, 38(1):54–83.

Argente, D., Lee, M., and Moreira, S. (2019). How do firms grow? the life cycle of products matters. *The Life Cycle of Products Matters (February 26, 2019)*.

Ashworth, S., Bueno de Mesquita, E., and Friedenberg, A. (2018). Learning about Voter Rationality. *American Journal of Political Science*, 62(1):37–54.

Baekgaard, M., Jensen, C., Mortensen, P. B., and Serritzlew, S. (2014). Local News Media and Voter Turnout. *Local Government Studies*, 40(4):518–532.

Baumeister, R. F., Bratslavsky, E., Finkenauer, C., and Vohs, K. D. (2001). Bad Is Stronger Than Good. *Review of General Psychology*, 5(4):323–370.

Bertrand, M. and Schoar, A. (2003). Managing with style: The effect of managers on firm policies. *The Quarterly journal of economics*, 118(4):1169–1208.

Blattner, L., Farinha, L., Rebelo, F., et al. (2018). When losses turn into loans: the cost of undercapitalized banks. *Banco de Portugal. Working Paper. Forthcoming*.

Blazenko, G. W. and Vandezande, K. (2003). Corporate holding of finished goods inventories. *Journal of Economics and Business*, 55(3):255–266.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D., and Roberts, J. (2013). Does management matter? evidence from india. *The Quarterly Journal of Economics*, 128(1):1–51.

Bloom, N. and Van Reenen, J. (2011). Human resource management and productivity. In *Handbook of labor economics*, volume 4, pages 1697–1767. Elsevier.

Bolton, P. and Scharfstein, D. S. (1990). A theory of predation based on agency problems in financial contracting. *The American economic review*, pages 93–106.

Bruhn, M., Karlan, D., and Schoar, A. (2012). 'the impact of offering consulting services to small and medium enterprises. *World Bank mimeo*.

Bruhn, M. and Zia, B. (2013). Stimulating managerial capital in emerging markets: the impact of business training for young entrepreneurs. *Journal of Development Effectiveness*, 5(2):232–266.

Bucă, A. and Vermeulen, P. (2017). Corporate investment and bank-dependent borrowers during the recent financial crisis. *Journal of Banking & Finance*, 78:164–180.

Campello, M., Graham, J. R., and Harvey, C. R. (2010). The real effects of financial constraints: Evidence from a financial crisis. *Journal of financial Economics*, 97(3):470–487.

Cancela, J. and Geys, B. (2016). Explaining voter turnout: A meta-analysis of national and subnational elections. *Electoral Studies*, 42:264–275.

Carpenter, R. E., Fazzari, S. M., and Petersen, B. C. (1998). Financing constraints and inventory investment: A comparative study with high-frequency panel data. *Review of Economics and Statistics*, 80(4):513–519.

Chemla, G. and Hennessy, C. (2019). Controls, belief updating, and bias in medical RCTs.

Chemmanur, T. J. and Simonyan, K. A. (2017). Top management quality, corporate finance, and corporate innovation. Technical report, ADBI Working Paper Series.

Chevalier, J. A. and Scharfstein, D. S. (1996). Capital-market imperfections and countercyclical markups: Theory and evidence. *The American Economic Review*, 86(4):703–725.

Cingano, F., Manaresi, F., and Sette, E. (2016). Does Credit Crunch Investment Down? New Evidence on the Real Effects of the Bank-Lending Channel. *The Review of Financial Studies*, 29(10):2737–2773.

Clark, N. and Rohrschneider, R. (2009). Second Order Elections versus First Order Thinking: How Voters Perceive the Representation Process in a Multi Layered System of Governance. *Journal of European Integration*, 31(5):645–664.

Custódio, C., Ferreira, M., and Matos, P. (2013). Generalists versus specialists: Lifetime work experience and chief executive officer pay. *Journal of Financial Economics*, 108(2):471–492.

Custodio, C., Ferreira, M. A., and Matos, P. (2017). Do general managerial skills spur innovation? *Management Science*.

Custodio, C. and Metzger, D. (2013). How do ceos matter? the effect of industry expertise on acquisition returns. *The Review of Financial Studies*, 26(8):2008–2047.

Custodio, C. and Metzger, D. (2014). Financial expert ceos: Ceo's work experience and firm's financial policies. *Journal of Financial Economics*, 114(1):125–154.

DellaVigna, S. and Kaplan, E. (2007). The Fox News effect: Media bias and voting. *Quarterly Journal of Economics*, 122(3):1187–1234.

Dimson, E., Rousseau, P. L., and Spaenjers, C. (2015). The price of wine. *Journal of Financial Economics*, 118(2):431–449.

Drexler, A., Fischer, G., and Schoar, A. (2014). Keeping it simple: Financial literacy and rules of thumb. *American Economic Journal: Applied Economics*, 6(2):1–31.

EBA (2011). Capital buffers for addressing market concerns over sovereign exposures - methodological note.

Eberl, J. M., Boomgaarden, H. G., and Wagner, M. (2017). One Bias Fits All? Three Types of Media Bias and Their Effects on Party Preferences. *Communication Research*, 44(8):1125–1148.

Epstein, R. and Robertson, R. E. (2015). The search engine manipulation effect (SEME) and its possible impact on the outcomes of elections. *Proceedings of the National Academy of Sciences of the United States of America*, 112(33):E4512–E4521.

Fazzari, S. M., Hubbard, R. G., Petersen, B. C., Blinder, A. S., and Poterba, J. M. (1988). Financing constraints and corporate investment. *Brookings papers on economic activity*, 1988(1):141–206.

Fee, C. E., Hadlock, C. J., and Pierce, J. R. (2013). Managers with and without style: Evidence using exogenous variation. *The Review of Financial Studies*, 26(3):567–601.

Field, E., Jayachandran, S., and Pande, R. (2010). Do traditional institutions constrain female entrepreneurship? a field experiment on business training in india. *American Economic Review*, 100(2):125–29.

Fiva, J. H. and Halse, A. H. (2016). Local favoritism in at-large proportional representation systems. *Journal of Public Economics*, 143:15–26.

Galasso, V. and Nannicini, T. (2016). Persuasion and Gender: Experimental Evidence from Two Political Campaigns. *CESifo Working Paper Series No. 5868*, (230088).

Garz, M. (2014). Good news and bad news: evidence of media bias in unemployment reports. *Public Choice*, 161(3-4):499–515.

Geys, B. and Vermeir, J. (2014). Party cues in elections under multilevel governance: Theory and evidence from US states. *Journal of the European Economic Association*, 12(4):1029–1058.

Gibbs, M., Tapia, M., and Warzynski, F. (2009). Globalization, Superstars, and Reputation: Theory & Evidence from the Wine Industry. *Journal of Wine Economics*, 4(01):46–61.

Gilje, E., Loutskina, E., and Murphy, D. P. (2017). Drilling and debt.

Giroud, X., Mueller, H. M., Stomper, A., and Westerkamp, A. (2011). Snow and leverage. *The Review of Financial Studies*, 25(3):680–710.

Graham, J. and Harvey, C. (2001). The theory and practice of corporate finance: evidence from the field. *Journal of financial economics*, 60(2-3):187–243.

Graham, J. and Harvey, C. (2002). How do cfos make capital budgeting and capital structure decisions? *Journal of applied corporate finance*, 15(1):8–23.

Graham, J. R., Harvey, C. R., and Puri, M. (2013). Managerial attitudes and corporate actions. *Journal of financial economics*, 109(1):103–121.

Gropp, R., Mosk, T., Ongena, S., and Wix, C. (2018). Banks Response to Higher Capital Requirements: Evidence from a Quasi-Natural Experiment. *The Review of Financial Studies*, 32(1):266–299.

Güner, A. B., Malmendier, U., and Tate, G. (2008). Financial expertise of directors. *Journal of financial Economics*, 88(2):323–354.

Haselmayer, M., Meyer, T. M., and Wagner, M. (2017). Fighting for attention. *Party Politics*, (January):1–12.

Hirshleifer, D., Low, A., and Teoh, S. H. (2012). Are overconfident ceos better innovators? *The journal of finance*, 67(4):1457–1498.

Howell, S. T. (2017). Financing innovation: Evidence from r&d grants. *American Economic Review*, 107(4):1136–64.

ISMEA (2017). The state of the european gi wines sector: a comparative analysis of performance.

IVV (2018). Abc do setor vitivinícola. *Instituto da Vinha e do Vinho*.

Iyer, R., Peydró, J.-L., da Rocha-Lopes, S., and Schoar, A. (2013). Interbank liquidity crunch and the firm credit crunch: Evidence from the 2007–2009 crisis. *The Review of Financial Studies*, 27(1):347–372.

Jackson, R. S. (2008). *Wine science: principles and applications*. Academic press.

James, O. (2011). Performance measures and democracy: Information effects on citizens in field and laboratory experiments. *Journal of Public Administration Research and Theory*, 21(3):399–418.

James, O. and John, P. (2007). Public management at the ballot box: Performance information and electoral support for incumbent english local governments. *Journal of Public Administration Research and Theory*, 17(4):567–580.

James, O. and Moseley, A. (2014). Does performance information about public services affect citizens' perceptions, satisfaction, and voice behaviour? Field experiments with absolute and relative performance information. *Public Administration*, 92(2):493–511.

Jensen, M. C. and Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of financial economics*, 3(4):305–360.

Kahn, K. I. M. F. and Kenney, P. J. (1999). Do Negative Campaigns Mobilize or Suppress Turnout ? Clarifying the Relationship between Negativity and Participation. *The American Political Science Review*, 93(4):877–889.

Kaplan, S. N., Klebanov, M. M., and Sorensen, M. (2012). Which ceo characteristics and abilities matter? *The Journal of Finance*, 67(3):973–1007.

Kappe, R. (2018). Asymmetric Evaluations : Government Popularity and Economic Performance in the United Kingdom. *Electoral Studies*, (September):13–15.

Karlan, D., Knight, R., and Udry, C. (2012). Hoping to win, expected to lose: Theory and lessons on micro enterprise development. Technical report, National Bureau of Economic Research.

Karlan, D. and Valdivia, M. (2011). Teaching entrepreneurship: Impact of business training on microfinance clients and institutions. *Review of Economics and statistics*, 93(2):510–527.

Kashyap, A. K., Lamont, O. A., and Stein, J. C. (1994). Credit conditions and the cyclical behavior of inventories. *The Quarterly Journal of Economics*, 109(3):565–592.

Kendall, C., Nannicini, T., and Trebbi, F. (2015). How Do Voters Respond to Information? Evidence from a Randomized Campaign How Do Voters Respond to Information? Evidence from a Randomized Campaign. *American Economic Review*, 105(1):322–353.

King, G., Schneer, B., and White, A. (2017). How the news media activate public expression and influence national agendas. *Science*, 358(November 10):776–780.

Kini, O., Shenoy, J., and Subramaniam, V. (2017). Impact of financial leverage on the incidence and severity of product failures: Evidence from product recalls. *The Review of Financial Studies*, 30(5):1790–1829.

Kramer, G. H. (1971). Short-Term Fluctuations in U . S . Voting Behavior, 1896-1964. *American Political Science Review*, 65(1):131–143.

Kremer, M., Rao, G., and Schilbach, F. (2019). Behavioral development economics. *Handbook of Behavioral Economics*, 2.

Krieger, J., Li, D., and Papanikolaou, D. (2018). Missing novelty in drug development. *NBER Working Paper*, (w24595).

Krüger, P., Landier, A., and Thesmar, D. (2015). The wacc fallacy: The real effects of using a unique discount rate. *The Journal of Finance*, 70(3):1253–1285.

Lau, R. R. (1982). Negativity in political perception. *Political behavior*, 4(4):353–377.

Lau, R. R. (1985). Two explanations for negativity effects in political behavior. *American Journal of Political Science*, pages 119–138.

Lewis-beck, M. S. and Stegmaier, M. (2000). Economic Determinants of Electoral Outcomes. *Annual Review Of Political Science*, 3:183–219.

Lockwood, B. and Rockey, J. (2015). Negative Voters : Electoral Competition with Loss-Aversion.

Lovell, M. (1961). Manufacturers' inventories, sales expectations, and the acceleration principle. *Econometrica: Journal of the Econometric Society*, pages 293–314.

Maksimovic, V. and Titman, S. (1991). Financial policy and reputation for product quality. *The Review of Financial Studies*, 4(1):175–200.

Malmendier, U. and Tate, G. (2005). Ceo overconfidence and corporate investment. *The journal of finance*, 60(6):2661–2700.

Malmendier, U. and Tate, G. (2008). Who makes acquisitions? ceo overconfidence and the market's reaction. *Journal of financial Economics*, 89(1):20–43.

Malmendier, U., Tate, G., and Yan, J. (2011). Overconfidence and early-life experiences: the effect of managerial traits on corporate financial policies. *The Journal of finance*, 66(5):1687–1733.

Marien, S., Dassonneville, R., and Hooghe, M. (2015). How Second Order Are Local Elections? Voting Motives and Party Preferences in Belgian Municipal Elections. *Local Government Studies*, 41(6):898–916.

Martins, R. and Veiga, F. J. (2013). Economic voting in Portuguese municipal elections. *Public Choice*, 155(3-4):317–334.

Matsa, D. A. (2011). Running on empty? financial leverage and product quality in the supermarket industry. *American Economic Journal: Microeconomics*, 3(1):137–73.

Mésonnier, J.-S. and Monks, A. (2014). Did the eba capital exercise cause a credit crunch in the euro area?

Moeller, J., de Vreese, C., Esser, F., and Kunz, R. (2014). Pathway to Political Participation: The Influence of Online and Offline News Media on Internal Efficacy and Turnout of First-Time Voters. *American Behavioral Scientist*, 58(5):689–700.

Myers, S. C. (1977). Determinants of corporate borrowing. *Journal of financial economics*, 5(2):147–175.

Nannestad, P. and Paldam, M. (1997). The grievance asymmetry revisited: A micro study of economic voting in Denmark,1986–1992. *European Journal of Political Economy*, 13(1):81–99.

Olsen, A. L. (2015). Citizen (Dis)satisfaction: An experimental equivalence framing study. *Public Administration Review*, 75(3):469–478.

Phillips, G. and Sertsios, G. (2013). How do firm financial conditions affect product quality and pricing? *Management Science*, 59(8):1764–1782.

Pocketbooks, E. (2011). Key figures on european business with a special feature on SMEs. *Eurostat, Luxembourg*.

Rotemberg, J. J. and Saloner, G. (1989). The cyclical behavior of strategic inventories. *The Quarterly Journal of Economics*, 104(1):73–97.

Schakel, A. H. and Jeffery, C. (2013). Are Regional Elections really Second Order Elections? *Regional Studies*, 47(3):323–341.

Selb, P. and Munzert, S. (2013). Voter overrepresentation, vote misreporting, and turnout bias in postelection surveys. *Electoral Studies*, 32(1):186–196.

Singer, M. (2011a). When do voters actually think "It's the Economy" ? Evidence from the 2008 presidential campaign. *Electoral Studies*, 30(4):621–632.

Singer, M. (2011b). Who Says "It's the Economy"? Cross-National and Cross-Individual Variation in the Salience of Economic Performance. *Comparative Political Studies*, 44(3):284–312.

Soroka, S. and McAdams, S. (2015). News, Politics, and Negativity. *Political Communication*, 32(1):1–22.

Soroka, S. N. (2006). Good News and Bad News: Responses Asymmetric Information to Economic Information. *The Journal of Politics*, 68(2):372–385.

Sutton, J. (2014). *An enterprise map of Mozambique*, volume 5. International Growth Centre in association with the London Publishing Partnership.

Taniguchi, M. (2016). The multi-store model for economic voting: Rome wasn't built in a day. *Electoral Studies*, 41:179–189.

Tilley, J., Neundorf, A., and Hobolt, S. B. (2018). When the Pound in People's Pocket Matters: How Changes to Personal Financial Circumstances Affect Party Choice. *The Journal of Politics*, 80(2):555–569.

Viana, R. C. and Rodrigues, L. L. (2006). A special accounting treatment for regulated industries? the case of the port wine industry. *Journal of Wine Research*, 17(1):11–34.